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STUDIES IN SUBSTANCE MEMORIZATION*

From the Department of Psychology of the Ohio State University

HORACE B. ENGLISH, E. L. WELBORN, AND C. D. KILLIAN

I. INTRODUCTION

It is sometimes forgotten that Ebbinghaus who began the scientific study of memory was an adherent of historic Associationism and that his famous experiments were conceived in the spirit of that School, which is so congenial also to common sense. The implication is that learning is to be regarded as the linking together of discrete units. Accordingly meaningless units, which are the most nearly discrete, should make the ideal experimental material. Hence the fateful invention of the nonsense syllable and the attempted rigorous ruling out of meaning as an irrelevant variable.

The vast majority of research since, though less explicitly concerned with the associationist doctrine and occasionally even opposed to it, has faithfully followed the experimental pattern set by Ebbinghaus. Of course it was experimentally impossible to eliminate significance and coherence altogether, but one could make allowances for it and go on as if it were not there. Of course, also, what might be called check experiments were conducted with meaningful material to see how the principles or "laws" discovered with meaningless material applied to ordinary learning. But in such experiments the procedure more or less completely followed the pattern of serial learning set by the nonsense syllable. Meaningful material was treated as if it were meaningless. One may almost say that every precaution was taken to insure that meaning or coherence should have as little influence as possible upon results. Thus has come about an experimental demonstration that *any learning, so far as it is a matter of the linkage of relatively neutral, self-sufficient "atoms,"* proceeds according to the "laws" or principles governing the learning of nonsense syllables.

But what if learning be not solely or even mainly a matter of

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joining together discrete units? Indications that this is the case are not wanting. Studies of "logical memory," and certain related researches, even though dominated by the experimental stereotypes of associationism, have revealed some interesting divergencies from the memory for nonsense syllables. Facility in learning seems to follow a graded series according to the type of material to be learned, with meaningless material at the bottom and organized meaningful material at the top. The enormously greater efficiency of "logical memory" is perhaps one of the most important facts in the whole field of learning, not only practically but theoretically. Yet we have not allowed these and other related differences to alter in any material way our general conception of the processes of learning. This is as if chemists were to find a form of "iron" which melted at half the customary temperature without attempting either to revise their concept of that element or to devise experiments which should lead to a different classification of the new substance.

A survey of the experimental data, on the other hand, has led us to the hypothesis that in learning, along side the processes of linking neutral item to item, we must put—as an essential factor and not as an accidental concommitant—the process (or processes) involved in apprehending the significance of what is to be learned. There is no need to exaggerate the novelty of this view. What we have sketched as the hitherto dominant trend influences even those who do not accept it, mainly because it has determined the lines of experimentation. But that learning involves something besides mere association by contiguity has, of course, been suggested before; and a number of familiar dichotomies—active *vs.* passive learning, intentional *vs.* incidental learning, insight or intelligent learning *vs.* trial-and-error, and eduction *vs.* reproduction—point with some directness toward the kind of distinctions we have in mind.[1] Without a many-sided experimental attack, however, any attempt at a clear-cut distinction and definition of the processes involved in substance learning is premature. It is our purpose in this series to initiate that attack.

It is by no means easy, however, to isolate "substance memory" for experimental study. Apprehension of significance seems always ac-

[1]Specifically our own thinking stems from McDougall and Smith (2), who develop one of the empirical implications of Bergson's theory of memory and turn it to experimental account.

companied by the more mechanical learning described as association by contiguity and apparently every effort to tie learning down to objectively measurable responses results in emphasizing the mechanical or rote aspects at the expense of the meaningful. (Consider, e.g., the implications of the method of retained members or of paired associates.) We may, of course, discern the effects of meaning by contrasting the learning of the relatively meaningless with that of the relatively meaningful. This has been done by other experimenters, though not in desirable detail and under varied conditions of learning and recall. But when we use learning tasks of differing degrees of meaningful organization, we thereby inevitably introduce uncontrollable variables which may or may not be corollary with the major variable to be studied. This difficulty has, we believe, infected all previous comparative studies of substance memory.[2]

We propose to avoid it by comparing, not two different learning tasks, but the relative facility of the substance and the rote learnings to be found in a single task. Our actual procedure implies, what will probably be easily granted, that verbatim memory is more largely a matter of linking together "neutral" items than is memory for the ideas in a passage. Our procedure, then, in the first investigations reported is to compare verbatim and substance memorizing for the same passage by the same persons under varied conditions of learning and retention. Thus we inquire whether the conditions for effective learning are the same when one learns the "substance" of a prose passage as when one learns the passage verbatim. In Section II the main variable considered is the effect of varied spacings in the learning period upon the two tasks, in Section III the main variable is the effect of the number of repetitions.

Answers to these questions are of importance for school practice as well as for theory. In many cases, "our sole purpose is to learn the essential content of a coherent context or a mass of concrete material without regard to the author's wording" (3). The effective conditions for such learning have, however, been tacitly assumed to be those which have been discovered for nonsense syllables or other kinds of exact memorizing. Even if this were true, it should not remain mere assumption.

---

[2]Except a very minor study of our own (1). As we plan a general summary of previous experimental work in logical and substance learning, to be published in the *Psychological Bulletin,* we shall not duplicate it here.

General Procedure. From ten to three hundred college students were assembled and caused, under fairly well controlled conditions, to study a passage of moderately difficult prose. A modification of the true-false test was used to measure recognition-memory. Some of the test items reproduced verbatim the words of the passage, while others stated the substance or meaning in entirely different language. The first kind of item could be answered correctly if the subject remembered either the meaning or the mere words, though almost certainly, under the conditions of the experiments, both factors were involved. With the second kind of item, the rigorous and systematic alteration of the wording gave primary effect to the subject's memory for the meaning. The two kinds of items thus permitted comparison of memory for the substance with verbatim memory of the same passage. Furthermore, in the several experiments there were systematic variations in the second type of test items; some of them tested memory for small "thought units," others for relatively large "thought units." The effects of the subjects' reorganization of the learning material were thus brought out. The technique was such, as will be shown later, that the differences to be discussed can not be attributed to any cause other than certain differences in the way the learner reacted to the task.

II. Verbatim and Substance Memorization With Varied Spacings of Readings[3]

Preliminary Experiments. Specially prepared articles averaging 1100 words in length on topics in psychology were read twice by groups of college students. Two tests, each of which consisted of eighteen true-false items, were used. One test dealt with specific facts, the other with comprehension of thought. The subjects were arranged in paired groups. Both groups of a pair studied the same two articles; one took a fact-test on one article and a comprehension-test on the other, while the other group reversed this arrangement. About one-third of the subjects were tested immediately after reading, another third at the end of twenty-four hours, and the remainder at the end of forty-eight hours. Somewhat more than 600 test records were obtained.

[3]This section is primarily the work of E. L. Welborn and is a condensation of a thesis offered in partial fulfillment of the requirements of the Ph.D. degree at the Ohio State University.

Reliabilities of the tests, calculated by the half-half method and the Spearman-Brown formula, were: for fact-tests, .60±.05; for comprehension tests, .34±.07.

Retention in case of the two kinds of tests at the end of the various intervals is shown by Table 1.

TABLE 1

RANGES AND MEDIAN SCORES IN THE PRELIMINARY SERIES

Groups	Fact-tests			Comprehension-tests		
	Number of subjects	Range of score	Median score	Number of subjects	Range of score	Median score
Immediate test	73	0-15	9.04	75	3-15	9.33
24-hour test	66	0-15	7.62	67	3-15	9.33
48-hour test	73	1-15	6.36	70	1.5-16.5	9.25

The loss in memory for facts was 15.7% in twenty-four hours, and 29.6% in forty-eight hours, but in memory for thought (or meaning) there was no loss worthy of mention.

As each subject took one fact-test and two comprehension-tests or vice versa, it was possible to correlate scores on pairs of similar tests (fact-fact or comprehension-comprehension) and on pairs of dissimilar tests (fact-comprehension). Eleven coefficients from similar pairs range from +.24 to +.74 with one exception of —.14, while seven coefficients from dissimilar pairs range from —.125 to +.03, with one exception of +.359. The difference in the two cases is clear-cut.

These results on retention and correlation were not sufficiently reliable to justify positive conclusions, but their consistency warranted the belief that reliable differences between the two contrasting learning tasks used could be obtained by a more careful use of the same or similar techniques.

Second Series of Experiments

Learning material and tests. The material consisted of three articles, averaging 1500 words in length and consisting of adaptations of published material on psychological topics. Two tests of the true-false type were prepared on each article. Fifty statements, hereafter called "verbatims," were prepared on each article. The verbatims were practically perfect reproductions of statements from the

article; the vocabulary of the passage was used throughout.[4] Then fifty statements, hereafter called "paraphrases," were made by rewriting the verbatims, using synonyms for the original words. Certain key words, however, were treated as common words and used in both kinds of statements, though never in such a way as to give a clue to the correct answer. The two lists of items were then divided in half, items being drawn alternately for each half. Then two tests were made by combining twenty-five items from each list so that each verbatim in one test corresponded with a paraphrase in the other and vice-versa.

A sample of the material will be given. A typical paragraph from one of the articles is the following:

> If pupils were not endowed with the tendency to organize and to systematize their knowledge and to make number meaningful, or the power of generalization, the task in arithmetic would be utterly hopeless. Manifestly it is impossible to teach arithmetic in such a way as to cover all number combinations. The advocates of specific drill are driven to admit that pupils can learn how to deal with many number combinations without specific drill upon them. This is equivalent to admitting that transfer of training plays an important part in learning arithmetic. Specific drill makes no provision for transfer of training.

Corresponding items in the two forms of the test are given in parallel columns below.

1. Children are naturally inclined to classify facts. (Paraphrase; true)	1. Pupils are endowed with the tendency to organize knowledge. (Verbatim; true)
2. The teacher of arithmetic can not hope to deal with all number facts. (Paraphrase; true)	2. It is impossible to teach arithmetic so as to cover all number combinations. (Verbatim; true)
3. Advocates of specific drill deny that pupils can learn how to deal with number combinations without specific drill upon them. (Verbatim; false)	3. Proponents of definite repetition believe that it is necessary to have definite repetition of every number fact. (Paraphrase; false)
4. Specific drill makes no provision for transfer. (Verbatim; true)	4. There is no place for transfer of training under the method of definite repetition of each number fact. (Paraphrase; true)

[4]Except for minor changes necessary to make some of the items disagree with the text; the samples on the next page are typical.

As a precautionary measure, data were secured on all three articles for each experimental variable in order to eliminate the influence of slight differences in difficulty upon the results.

Learning groups and procedure. The learning groups were made up of classes in educational psychology at the Ohio State University. A special study of these groups showed that they were approximately equal in the sorts of learning ability utilized in these experiments. Furthermore, each group was divided into approximately equal halves on the basis of percentile rank on the Ohio State University Psychological Examination and one half of each group was tested with one test form, and the other half with the second form.

Four readings were provided for learning. To regulate the rate of presentation, the method of silent reading by the learners accompanied by oral reading by the experimenter was used. Each learning group was given three articles, with different spacing of reading on each. The spacings were:

1. Four consecutive readings.
2. Four readings at three-hour intervals.
3. Four readings at twenty-four-hour intervals.
4. Four readings at three-day intervals.

The plan, which has been described, equalized all factors in the situation except the two contrasting recognition tasks under varied spacings of readings. For each of the variables mentioned the number of test records obtained was between seventy-five and one hundred. Altogether somewhat over 1000 records were obtained.

Several groups were given an interpolated test at the end of the second reading, while the others were not. All groups were tested immediately after the fourth reading and again four weeks later. In two special groups an additional test was given at the end of eight weeks.

Reliability of tests. By the half-half method and use of Spearman-Brown formula the following coefficients of reliability for tests on the three passages were obtained: $.57 \pm .06$, $.65 \pm .04$, and $.66 \pm .04$.

Scores on identical tests given four weeks apart correlated from $.464 \pm .06$ to $.609 \pm .05$.

A thorough consideration of the question of test reliability led to the conclusion that these coefficients were satisfactory for com-

parisons among the large groups used. The reliability was deemed too low for individual comparisons, and none were made.

Comparison of control and experimental groups. The control group was one which took the tests without having read the material. A comparison of the results for this group and those for the experimental groups showed that on verbatims only 3.1% of the controls exceeded the median of the experimental group, and on paraphrases 1.56% did so. Accordingly, the scores of the experimental group appear to have been largely due to the effect of the readings.

Comparison of verbatim and paraphrase memory. The principal results are given in Table 2.

TABLE 2

MEDIANS, STANDARD DEVIATIONS, DIFFERENCES BETWEEN MEDIANS, AND CRITICAL RATIOS OF DIFFERENCES OF THE FOUR SPACINGS OF READING

Groups	Medians		S.D.'s		Diff. between medians of		Diff./P.E.$_{Diff.}$
	V.	P.	V.	P.	V.	P.	
			Test 1 (immediate)				
Unspaced	18.5	16	3.0	3.6	2.5		4.5
3-hr. spacing	20.1	17.4	2.9	3.3	2.7		5.0
1-day "	19.1	16.7	2.8	3.3	2.4		4.7
3-day "	17.4	16.4	2.9	2.2	1.0		2.3
			Test 2 (after four weeks)				
Unspaced	16	14.7	3.5	3.3	1.3		2.2
3-hr. spacing	17.4	16	3.5	3.9	1.4		2.3
1-day "	18	15.4	3.4	3.2	2.6		4.5
3-day "	15.9	15	2.7	2.7	0.9		1.9

V = Verbatim
P = Paraphrase

These data indicate that in all distributions the scores on verbatims were higher than on paraphrases in the same tests. In two spacings the forgetting of verbatims was noticeably greater in four weeks than that of paraphrases,[5] while in the other two spacings forgetting was practically the same in case of the two kinds of items. For the most part the differences may be regarded as statistically significant.

On the other hand, a correlational study failed to show any

[5]As shown by differences between corresponding medians of Test 1 and Test 2.

marked difference between coefficients from scores on pairs of like tests and those from scores on pairs of unlike tests (verbatim *vs.* paraphrase). The mean of all coefficients was .48, with a very small deviation.

Comparison of results on various spacings of readings. Table 2 gives the principal results. A comparison of the results of each spacing with those on all the others indicates that, for our material and procedure, the results on both learning and retention are consistent in showing that there is an optimal spacing somewhere between unspaced readings and readings three days apart. Readings at three-hour intervals gave best results in all tests with one exception, and in the latter case daily readings were slightly better. The learning scores on verbatims yielded critical ratios above three between the optimal spacing and both unspaced readings and three-day spacing, and the retention scores yielded parallel critical ratios slightly below three. The critical ratios yielded by paraphrase scores on learning and retention were somewhat more than two.

Effect of interpolated tests. The results are given in Table 3.

TABLE 3

COMPARISON OF MEDIAN SCORES OF GROUPS HAVING MID-TEST WITH THOSE OF GROUPS WITHOUT MID-TEST

Groups	No. cases	Test 1 (immediate)			Test 2 (after 4 wks)		
		Without mid-test	With mid-test	Diff.	Without mid-test	With mid-test	Diff.
Verbatim	58	18.2	19.4	1.2	17.5	18.8	1.3
Paraphrase	58	16.35	17.75	1.4	15.3	17.2	1.9
Verbatim	30	19.5	20.5	1.0	19.1	19.5	0.4
Paraphrase	30	18.1	17.9	—.2	17.0	18.0	1.0

Since these results are consistent throughout, with one slight exception, and since the four differences in case of the group of fifty-eight subjects yielded critical ratios ranging from 2.3 to 3.81, with a mean of 2.82, it is probable, though not certain, that the differences were due to the interpolated test.

Retention at the end of eight weeks. The learning groups available for this more extended period were too small for satisfactory comparison of verbatim and paraphrase retention, but the results are of special interest, because they show that the rate of loss was about the same the second four weeks as the first four.

Third Series of Experiments. There were two special purposes

in carrying out this new series of experiments: (1) To compare the results of learning when the tasks differ to a greater degree than those of the Second Series; and (2) to increase the reliability of the tests used for comparison.

Learning material. Two of the passages previously used were rewritten and extended in length by about 75%. One test upon each passage, composed of eighty items instead of fifty, as in the preceding Series, was prepared. Fifty of the items were verbatims exactly like those previously used. Twenty-five were like the former paraphrases in the use of a vocabulary different from that of the passage, but they differed in scope. They were summaries or topic sentences covering sections of varying length in the passage. Furthermore, five items not covered by the passages were included, solely to provide a third alternative for the learner in marking the test items. The instructions were to mark each item as agreeing with passage, not agreeing, not derived, or not remembered. The score was the number of items correctly marked as agreeing or disagreeing.

A sample paragraph from one passage follows:

> The accumulation of knowledge concerning the feeble-minded by the dawn of the 20th century had led to the clearly formulated conclusion that the feeble-minded are incapable of self-directed lives. Since then the advance in diagnostic methods has resulted in the detection of higher grades of feeble-mindedness; this, in turn, aroused hope on the part of many investigators that the high-grade feeble-minded at least may be capable of being educated sufficiently to meet the demands of society. On the other hand it has been shown that only a pitifully small percentage of feeble-minded make good when they are replaced in the community after being in an institution for the feeble-minded.

The summary based on this passage was as follows:

> "Views as to the educability of mental defctives were less harmonious about 1900 than they have been since that time." (False)

The sample verbatims given in the Second Series will serve also for this Series.

Learning groups and procedure. Several classes in educational psychology at the Ohio State University, having a total of 218 stu-

dents, were used. None of them had been used in either of the preceding series.

The mode of presentation was the same as that of the Second Series, except that it was necessary to reduce the number of readings to two. Half of the learners were given unspaced readings of the first passage and readings four days apart of the second passage while the other half were given readings of the first passage four days apart and unspaced readings of the second passage. This was done to eliminate any effect on scores due to difference in difficulty of the passages. Tests were given on each passage immediately after the second reading, again at the end of twenty-four hours, and again at the end of four weeks. Somewhat more than 900 test records were obtained.

Reliability of tests. By the half-half method and use of the Spearman-Brown formula, reliability coefficients of .767±.02 and .82±.02 for the two tests were obtained. It is evident that the reliability was considerably higher than that of the tests of the Second Series and was fairly high for group comparisons, which were the only kind made.

Scores on the same test given at different times correlated as indicated in Table 4.

TABLE 4

CORRELATIONS OF SCORES ON SAME TEST GIVEN AT DIFFERENT INTERVALS

Passage	Between tests 1 and 2	Between test 1 and 3	Between tests 3 and 2
Summaries, pass. F.	.722±.03	.565±.05	.493±.05
Verbatims, " F.	.884±.02	.691±.04	.745±.03
Summaries, pass. N.	.741±.03	.296±.06	.420±.05
Verbatims, " N.	.664±.04	.485±.05	.577±.04

In the above table, Test 1 indicates a test immediately after reading; Test 2, one twenty-four hours later; and Test 3, one four weeks later. Particular attention is called to the high correlations of tests twenty-four hours apart; they compare very favorably with parallel results from numerous published studies.

Comparison of results of control and experimental groups. The control group was one which took the tests without having read the passages. A comparison of their results with those of the experimental group showed that, in case of summaries, 44% of the controls exceeded the median of the experimental group, and in case of

verbatims, 12%. While the great amount of over-lapping in case of summaries is unfortunate, it is believed that the experimental group gained more from the readings than the data indicate. It is possible that the controls utilized criteria in answering which, if they had been used also by the experimental group, would have raised the scores of the latter. An item analysis of the summaries was made, but it threw little light on the situation. On the other hand the reliability of the summaries in the experimental group gives some support to the explanation suggested above. The half-half correlation of summaries alone, raised by the Spearman-Brown formula, is .78 for a test as long as the whole test, as against .82, which was the coefficient found for the summaries and verbatims combined. Mean correlations of scores on the same test twenty-four hours apart were for summaries .731, for verbatims .774. We do not wish, however, to over-emphasize these reliability data, and we admit that some allowance for the fact that previous knowledge and mere chance play too large a part in these summary items must be made in interpreting the experimental results.

Comparison of results of learning in case of summaries and verbatims. Table 5 gives the results on the complete data for all subjects. The same results are shown graphically in Figure 1.

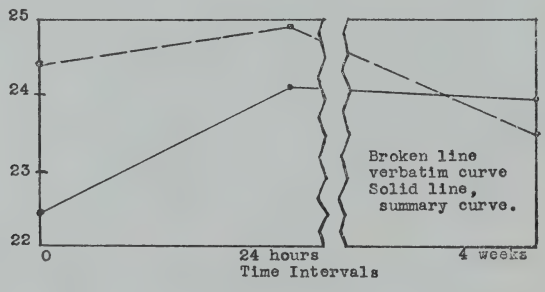

FIGURE 1
RETENTION CURVES FOR VERBATIMS AND SUMMARIES

It should be noted that, in Test 1, the scores on verbatims were higher than those on summaries. The critical ratio of the difference was found to be 4.53, and accordingly the difference is to be regarded as significant. The curves for both kinds of items rise between Tests 1 and 2, the rise in the curve of summaries being

TABLE 5

MEDIANS, THEIR DIFFERENCES, AND STANDARD DEVIATIONS FOR SUMMARY AND VERBATIM TESTS, BOTH SPACINGS COMBINED, 261 CASES

Kind of items	Test 1	Test 2	Test 3
	Medians		
Verbatim	24.4	24.9	23.4
Summary	22.4	24.1	23.9
Difference	—2.0	—0.8	0.5
	Standard deviations		
Verbatim	5.4	5.5	
Summary	6.6	6.8	
Difference	—1.2	—1.3	

relatively greater. Between Tests 2 and 3 the curve for summaries shows practically no loss, but the curve for verbatims drops enough to fall below the other by the end of four weeks. The critical ratio of the difference between the two distributions on Test 2 is 3.73, thus indicating a significant difference. Though the difference in Test 3 is very small, it should be remembered that it is in the opposite direction from the differences in the preceding tests.

Another method of comparison was used. The scores on pairs of like tests (V-V and S-S) and on unlike tests (S-V) twenty-four hours apart were correlated. The array of coefficients is given in Table 6.

TABLE 6

CORRELATIONS OF SETS OF LIKE AND UNLIKE TEST SCORES

Like pairs	Pearson *r*.	Unlike pairs	Pearson *r*.
Pass. F, V_1 & V_2	.884±.02	Pass. F, V_1 & S_2	.089±.04
Pass. N, V_1 & V_2	.664±.04	Pass. F, S_1 & V_2	.506±.05
Pass. F, S_1 & S_2	.722±.03	Pass. N, V_1 & S_2	.419±.06
Pass. N, S_1 & S_2	.741±.03	Pass. N, S_1 & V_2	.435±.05

The mean correlation of pairs of tests on summaries is .73, of pairs of tests on verbatims is .774, while that of pairs of unlike tests is .487.

The two types of comparison of verbatim and summary learning give results that warrant the conclusion that there are important differences between the two kinds of learning.

Spacing of readings. No important differences were found be-

tween the results of the two spacings used. In only one comparison out of six was there a difference having a critical ratio as large as three. These results are consistent with those on spacing in Series II. In that Series it was found that unspaced reading and three-day spacing gave very similar results, and both spacings were clearly inferior to three-hour spacing.

Fourth Series of Experiments. One of the passages used in the Third Series was studied three times (instead of two) by a different group of 85 college students. Figure 2 shows the results of the

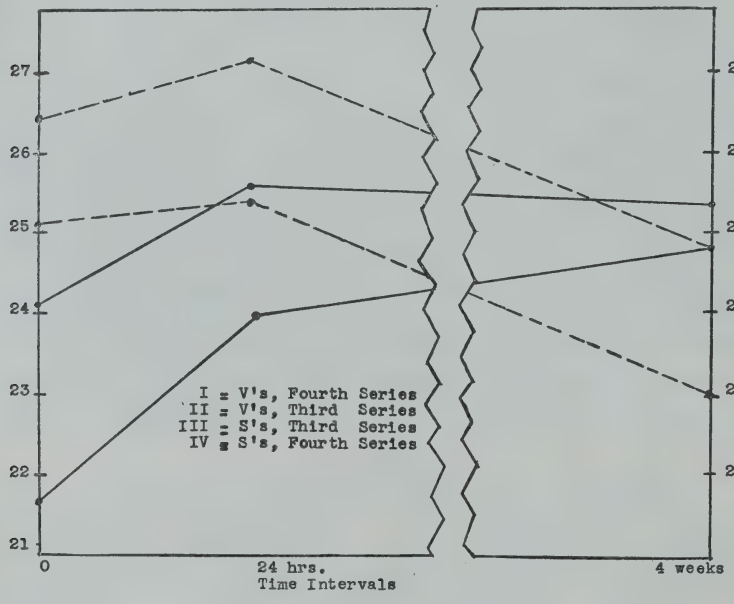

FIGURE 2
RETENTION CURVES FOR THIRD AND FOURTH SERIES

Fourth Series and also the portion of the results of the Third Series derived from the same passage. In the Fourth Series there was a somewhat greater difference than in the Third Series between verbatim and summary scores on the initial tests, possibly due to the extra reading. The more important result is that, as before, the verbatim scores showed a significant loss after four weeks, while the summary scores showed a small but noticeable gain. Similar curves

in the two series are practically identical in form. The differential effect of the lapse of time on the two tasks is most evident.

Summary

1. In the Second Series differences, for the most part reliable, among various spacings were clearly favorable to an inter-practice interval of from three to twenty-four hours. In the Third Series the results, though not clear-cut, were consistent with those of the Second. There were no reliable differences between the two kinds of recognition-task, the Verbatim and the Non-Verbatim. The effect of moderate spacing is greater for immediate than for delayed recognition.

2. Immediate recognition was inferior with both kinds of items to recognition after twenty-four hours.

3. There is less loss in the recognition of paraphrase items than of verbatim items based on the same passage and studied under the same conditions (Series II). There is an even larger difference of the same sort when the comparison is between summary items and verbatims. In fact in one case (Series IV), there is no apparent forgetting but even actual gain in summaries scores after four weeks without intervening study.

III. The Effect of the Number of Readings Upon Substance and Verbatim Memory of the Same Material[6]

Fifth Series. The test prepared paralleled very closely that described in the Second Series. Two parallel test forms were thus constructed, each of 25 V- and 25 P-items. One half of each experimental group took one form of the test, the other half the other form. The subjects were 134 students in an elementary course in educational psychology. They were assembled to hear the passage read to them, and were divided by a random selection into five groups. One group heard the passage read once; another group heard it read twice, another three, another four, and one group heard it read five times. Speed of reading, expression, loudness, and such factors were fairly constant. The motivation of the subjects was in general good. After each reading the students who were to constitute that experimental group went to another room

[6]This section is primarily the work of C. D. Killian and is a condensation of a thesis presented to the Faculty of the Ohio State University in partial fulfilment of the requirements for the Ph.D. degree.

and after about 10 minutes took a test on the material they had just heard.

They were retested at intervals of 1, 8, and 71 days, each student taking the same form of the test on the later occasions as on the first. All the later tests were unannounced. Of the original 134, 91 students completed all three tests. Unless otherwise stated, data are given in what follows only for these latter. The reliability of the test was determined by both the split-half correlation technique and the repetition-of-test technique. The corrected coefficients obtained by the split-half technique vary from .47±.074 to .65±.063. The coefficients obtained by repetition-of-test technique vary from .71± .0565 to .86±.035.[7] These were believed to be sufficiently reliable for group comparisons; certainly they do not suffer by comparison with similar tests.

A control group of 33 students took the test without having heard the passage read. The median scores of this control group were for V-items 5.79 and for P-items 7.17. These may be contrasted with medians of 13.37 and 10.41 for the experimental groups, showing that a significant amount of learning was induced by the study allowed. As was to be expected, when the test followed the presentation after a few minutes interval, the V-items were the more readily recognized.

Table 7 shows the results for immediate memory of the differing numbers of repetitions. From this table it can be seen that in the case of V-learning an increase in the number of readings beyond two certainly does not bring an increase in score. As many

TABLE 7

THE MEDIAN SCORES FOR THE VARIOUS READING GROUPS—TESTED 10 MINUTES AFTER STUDY

	Number of readings				
	1	2	3	4	5
V-score	17.5	19.33	19.33	18.0	19.08
P-Score	17.13	17.5	18.0	18.9	17.38

N=124. (All persons taking the first test are included.)
Possible score=25.

[7]We have no explanation of the fact that the re-test correlations were so consistently higher. This is contrary to the usual experience, yet seems hardly an accident.

as two readings do seem to be of value. In the case of P-learning there are no significant differences.

The facts bearing on retention are shown in Figure 3. Recognition

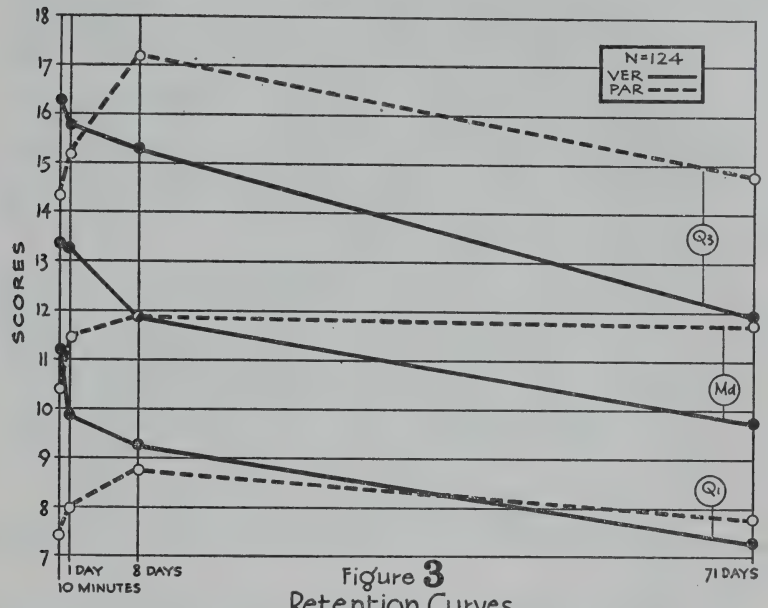

Figure 3
Retention Curves
in Terms of Quartiles
SERIES I

for the V-items falls off consistently from the first to the seventy-first day. On the other hand, recognition of the P-items shows a consistent rise. Starting materially lower, the curves cross the retention curve for V-items at about the eighth day. By the seventy-first day, the difference between the two medians is 4.8 times the probable error of the difference.

Sixth Series of Experiments. In the fifth series of experiments no differential effect of number of repetitions was found on the two types of test-item. A possible explanation of this is that we had not departed far enough from verbatim learning in our paraphrase items. We decided therefore to make statements which should summarize the thought of a whole paragraph.

Due to the nature of the items only one test form was used. There were twenty summary items (called S-items) similar to those used in the Third Series, sixty verbatim items (V-items), and twenty items which were statements of such similar general tenor that they might reasonably be thought to have been in the passage, but were not (N-items). The subjects were instructed (before being tested but *after* study of the passage) as follows: "If a statement below conveys the same idea, whether or not in the same words, as is conveyed in the passage, mark it A for agrees. If it conveys an idea which is not in the passage at all mark it N." In comparisons, unless otherwise noted, the S-scores were multiplied by three in order to make them equivalent to the V-scores.

Three hundred and ninety-one elementary psychology students took part in this series of experiments, which were conducted during the spring and summer quarters of 1932. A prose passage of 2150 words was read to them as they followed along reading silently. They were divided on a random basis into four reading groups, studying the passage respectively one, two, three and four times. The test was given immediately after the reading and at intervals of 1, 14, and 30 days. The same test was used on the later occasions and the tests were unannounced. One class took the test without having heard the passage read and was used as a control group.

The reliability coefficient of the test by the split-half technique based on the first 117 cases completing all the tests was found to be $.82 \pm .0204$.

By comparison with the control group it was found that on the V-scores no one in the control group surpassed the median of any of the experimental groups; on the S-scores 15 per cent of the control group surpassed the median of the experimental group.

In this series we have shown data separately for number of readings and also for all groups combined. The former are presented in Figure 4 and the latter in Figure 5. The S-items are much harder than the P-items of Series V, hence a greater initial difference between V and S. But the same phenomena appear as before: loss in one, gain in the other type of item with the passage of time. Furthermore, we seem to find here a difference between V- and S-items due to differences in the number of repetitions, though it is so complicated a comparison that we do not wish to lay stress on it.

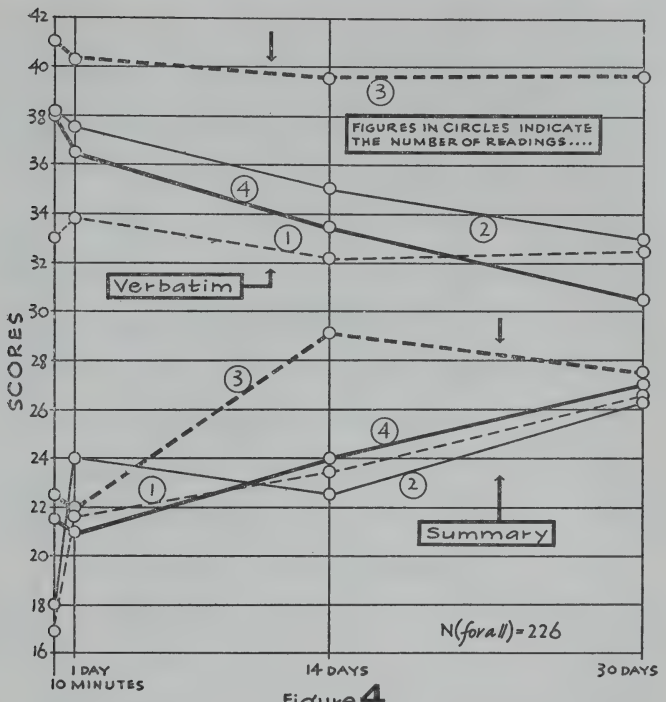

Figure 4
Retention Curves in Successive Tests
SERIES II

Reference for the following discussion should be made to Table 8. In Test 1, taken almost at once after the readings, two readings are clearly better than one (the critical ratio of the difference is 3.47) for V-items. A day later (Test 2) the difference is less, 8 days later still less, 30 days later it is negligible. A parallel change is found when we consider the differences between one and four readings. Under our conditions two or four readings are about equally better than one for the purposes of immediate recognition. Three readings, however, are better at all points taken. This is true, however, only for V-items; S-items show no significant differences nor any consistent trend in favor of any one number of readings save that three seems slightly the best. All comparisons, moreover, between V and S in respect to the effect of repetitions are stronger when

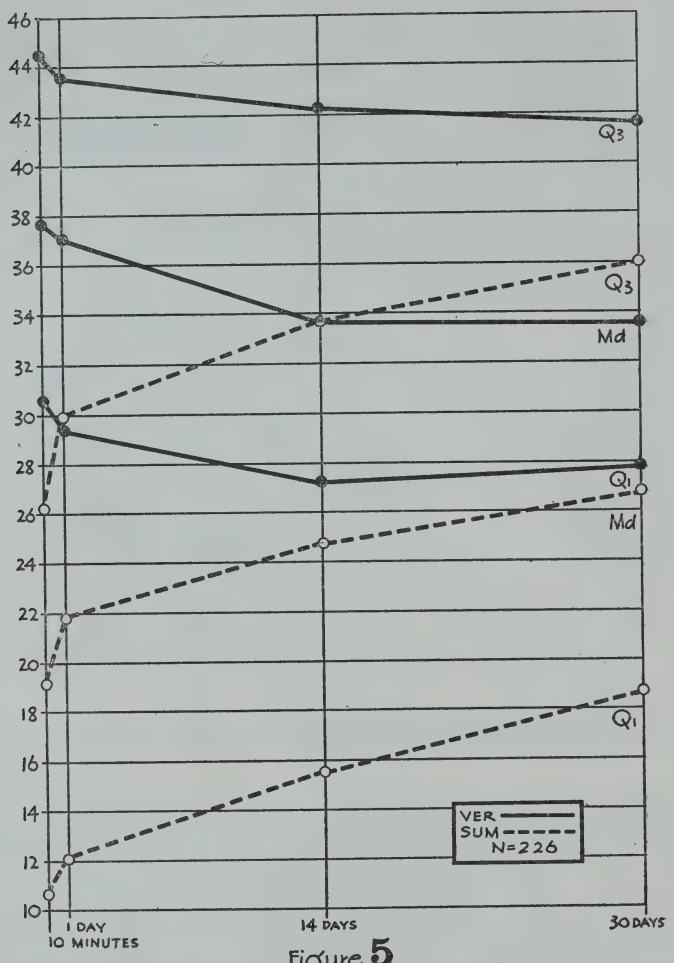

Figure 5
Retention Curves
in Terms of Quartiles
SERIES II

made for relatively immediate memory. As the time interval is extended, V- and P-items become more alike, not in absolute score, but in respect to the influence upon them of the number of repetitions. Summarizing: it makes practically no difference how many

TABLE 8

EFFECT OF THE NUMBER OF READINGS UPON THE MEDIAN V- AND S-SCORES AS DETERMINED BY THE RELIABILITY OF THE DIFFERENCES

Groups	I & II	II & III	III & IV	I & III	I & IV	II & IV
			Test 1—(10 minutes after reading)			
			Verbatim items			
Diff.	5.17	2.83	—3.00	8.00	6.66	—.17
P.E.D.	1.49	1.50	1.90	1.67	1.88	1.74
D/PED	3.47	1.89	1.58	4.79	3.54	.098
			Paraphrase items			
Diff.	.36	1.5	—.33	1.86	1.43	1.17
P.E.D.	.493	.378	.583	.518	.499	.561
D/PED	.73	2.59	.57	3.59	2.87	2.09
			Test 2—(1 day later)			
			Verbatim items			
Diff.	3.67	2.75	—3.75	6.42	2.67	—1.00
P.E.D.	1.49	1.53	2.00	1.63	1.97	1.89
D/PED	2.46	1.78	1.87	3.94	1.36	.53
			Paraphrase items			
Diff	.80	—.77	—.33	.13	—.20	—1.00
P.E.D.	.641	.653	.720	.637	.730	.745
D/PED	1.25	1.18	.46	.21	.27	1.34
			Test 3—(8 days later)			
			Verbatim items			
Diff.	2.80	4.5	—6.0	7.30	1.30	—1.5
P.E.D.	1.56	1.86	1.93	1.79	1.64	1.74
D/PED	1.79	2.42	3.11	4.08	.79	.88
			Paraphrase items			
Diff.	—.30	2.21	—1.71	1.91	.20	.50
P.E.D.	6.57	.669	.662	.691	.649	.626
D/PED	.46	3.30	2.58	2.76	.31	.80
			Test 4—(30 days later)			
			Verbatim items			
Diff.	.5	6.66	—9.16	7.16	—.20	—2.5
P.E.D.	1.39	1.53	2.03	1.41	1.94	2.03
D/PED	.36	4.35	4.51	5.08	1.03	1.23
			Paraphrase items			
Diff.	—.09	.22	.17	.13	.30	.39
P.E.D.	.577	.708	.932	.687	.837	.854
D/PED	.16	.31	.18	.19	.36	.46

repetitions (up to 4) one has if we consider the ability to recognize the general drift of the passage; it does make a difference if we consider ability to recognize correctly sentences which reproduce the original, verbatim. As we get further away from the learning period, however, the influence of the exact verbatim wording decreases and accordingly the value of additional repetitions is less.

Another approach is the correlational. If the verbatim and summary tests were measuring the same process we should expect the correlations between them to be as high as the split-half reliability coefficients. If, on the other hand, we find a high reliability coefficient and a low correlation between V- and S-scores it would be indicative that these two tests are measuring different processes. The correlation between V- and S-scores on the first 117 cases was .39. The reliability coefficient[8] on the same group was .69. The difference is $.30 \pm .055$, its critical ratio 5.45, which according to the usual criterion is ample. Here then we have two measures of the same *identical* material studied under *identical* conditions, yet yielding markedly different results. The inference that distinct factors are operative is clear.

Seventh Series. It is well known that recitation is a considerable factor in learning. It is quite clear that each testing period is in effect such a recitation. It was thought that this might account for the actual increase in score of the S-items, though one would still have to account for the fact that the V-items showed a loss. In any case, using the technique of Series VI we administered the test to 152 students directly after reading the passage 1, 2, or 3 times. No further test was given for 55 days. We find the same decline in V-items and the same increase in S-items. (See Figure 6). These results also confirm the conclusion of Series VI respecting number of repetitions.

Eighth Series of Experiments. The most obvious explanation of the absolute increase in S-scores is that the subjects were studying analogous material, with high transfer value. This explanation leaves one at a loss to explain the decline in V-scores, since the same content was involved. Be that as it may, we determined to check on the transfer hypothesis. A group of 30 students took the test

[8]Not raised by the Spearman-Brown formula as the number of items in the cases is the same.

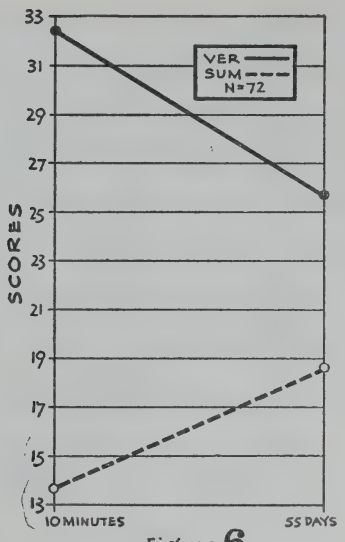

Figure 6
Curves of Retention in V and S Scores over an Interval of 55 days

without prior reading of the passage. The median scores were S=6.00 and V=15.00. Fifty-five days later they were tested again. A small increase in scores (medians: S=8.23 and V=16.00) was found but this was true for V and S to about the same extent. Thus neither the gain in S- nor the loss in V-scores is attributable in any marked degree to what the subjects learned in the ordinary course of events between one test and another. (See Figure 7.)

Ninth Series of Experiments. Here 20 students in an advanced course in psychology were given the test without prior reading of the passage and were then given five days within which to learn the passage in any way they liked, to "complete mastery." The test was then repeated 47 days later. The "mastery" attained was not in fact complete but there were strikingly higher scores (V-items 47.5 out of 60, S-items 30.9 out of 60 as compared with 37.64 and 19.23 for the students of Series VI). The same general phenomena were found here as in Series VI, that is to say, there is a gain in S-items and a loss in V-items. (See Figure 8.)

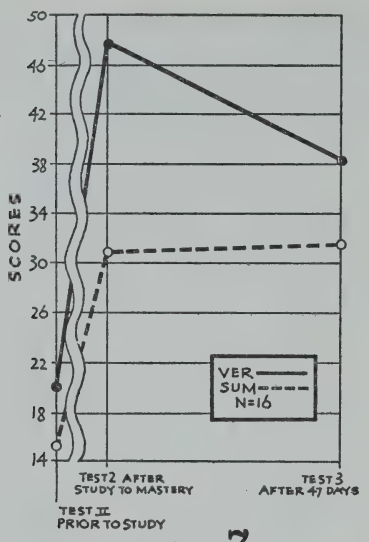

Figure 7
Retention when the Learning of the Passage Approaches the Point of Complete Mastering

Summary

1. Repetition, within the limits of the conditions of our experiments, favorably affects immediate and delayed recognition-memory of verbatim items. It seems to have little or no effect upon the recognition of summary items.

2. Correlations between V- and S-scores are lower than the reliability coefficients, indicating a measurable lack of community between the two sorts of items.

3. Retention curves for the two sorts of items, V and S, consistently show opposed trends. Those for V-items show a typical Ebbinghaus decline; those for S-items do not look like forgetting curves at all but like learning curves. This rise, however, takes place without intervening study or (in Series VII) recitation.

IV. General Conclusions and Discussion

1. The first conclusion which we draw is methodological: the learning parameter is a function not only of the external conditions

imposed and of the motivation of the subject, but also and very distinctly of the technique of measurement. The identical bit of learning, externally regarded at least (and in view of our ignorance about the basic or underlying processes we are almost compelled to take learning externally), may be made to show a rising or falling curve of retention by a shift in the measuring technique. The point, however obvious it may seem, is almost universally ignored in interpreting experimental data.

2. Certain practical implications deserve emphasis. Working with a kind of learning which resembles actual academic study fairly closely, we confirm previous investigations which favor at least a moderate spacing of "practice." Closely connected with this is the evidence that tests such as those used by us are themselves teaching devices of much merit when given at short intervals after a given unit of learning has been completed.

The outstanding differences between the Verbatim and the Summary test items lead also to a practical conclusion. There is a widespread opinion, implicit in most school and college examinations (by no means confined to the so-called new-type examination), that tests of verbatim learning are adequate tests of substance learning. Our results flatly deny this implication. Examination techniques must directly measure the kind of learning desired—and the Summary-items seem to us examples of one way to measure objectively a highly desirable form of learning.

Carrying this line of thought a little further, it seems to us evident that substance memorizing must be pretty thoroughly worked over independently and by experimental techniques which emphasize, rather than by those which minimize, its characteristic features. Our results give color to the prediction that such investigation will bring to light many new principles of fundamental importance about the most significant manifestation of learning.

3. Finally, we invite attention to the implications of this study for learning theory. Traditionally, learning has been conceived as the formation and fixation of routine, rote responses. Experimental literature is full of investigations which seem to validate this conception. Educational philosophers, however, under the leadership of Dewey have attacked it as sterilizing to education, and the Gestalt School seems to deny that learning can proceed at all in such associative-synthetic manner.

Our data, on the other hand, strongly suggests that our Verbatim-items measure predominantly just such rote associative-synthesis in learning while our Summary-items reflect a greater predominance of another factor which it is premature to delimit exactly or to name without further experimentation, but which is tied up with the organized or coherent or meaningful in experience or behavior. This comes out most clearly in the curves of retention. The same subjects, learning the same material at the same time and under the same conditions, on the one hand forget rapidly, or on the other retain what is learned, depending upon whether our criterion of learning is a fixed routine response or a grasp of the meaning or substance. The differential effect of frequency points in the same direction. Successive repetitions seem to yield new and deeper meanings rather than to fixate those already comprehended, but additional repetitions seem to fixate verbatim responses somewhat as asserted by associationism.

Our present data are admittedly inconclusive; they serve rather to indicate the explanatory possibilities of the hypothesis than to establish it, and to indicate the value of further experimentation. Thus present ignorance, practical needs, and the promise of theoretical development all point to the field of substance learning as ripe for exploitation.

V. Summary

College students were tested for their recognition-retention of a passage of difficult prose by two contrasted types of test items: one reproducing almost verbatim portions of the text, the other requiring the recognition of the meaning when couched in entirely different words. Frequency of repetitions (from 1 to 4 readings of the text) affected the verbatim items but not the non-verbatims. Recognition of both kinds of items was better after 24 hours than immediately, but verbatim items showed an Ebbinghaus-like curve of forgetting thereafter while the non-verbatim items were apparently not forgotten at all over periods up to 70 days and in some cases showed a statistically reliable gain. These results are taken to indicate that substance learning differs not only from the learning of nonsense syllables but also from so-called logical learning in which the words of the original text are preserved. Need for an independent study of the learning of the substance is thus indicated. The hypo-

thesis is proposed that learning involves the close and inseparable cooperation of at least two processes: the one emphasized in verbatim learning and in the conditioned response, the other in "insight," the learning of substance.

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Ohio State University
Columbus, Ohio

ÉTUDES DE L'APPRENTISSAGE PAR COEUR DU CONTENU

(Résumé)

On a testé des étudiants universitaires pour leur reconnaissance-rétention d'un passage de prose difficile au moyen de deux types contrastés de test: l'un reproduisant presque mot pour mot des portions du test, l'autre exigeant la reconnaissance de la signification exprimée en mots tout à fait différents. La fréquence des répétitions (1-4 lectures du texte) a influé sur les portions mot pour mot mais non pas sur les autres portions. La reconnaissance des deux types a été meilleure après 24 heures que tout de suite, mais les portions mot pour mot ont montré après une courbe d'oubli semblable à celle l'Ebbinghaus tandis que les autres portions n'ont pas été oubliées, paraît-il, dans les périods durant jusqu'à 70 jours et en quelques cas ont montré un gain statistiquement constant. On suppose que ces résultats indiquent que l'apprentissage du contenu diffère non seulement de l'apprentissage des syllables non-sens mais aussi de l'apprentissage soi-disant logique où les mots du texte original sont préservés. Il s'indique ainsi la nécessité d'une étude indépendante de l'apprentissage du contenu. On propose l'hypothèse que l'apprentissage implique le coopération proche et inséparable de deux procédés du moins: l'un dominant dans l'apprentissage mot pour mot et dans la réponse conditionnelle, l'autre dans la "compréhension," l'apprentissage du contenu.

English, Welborn et Killian

STUDIEN ÜBER DAS INHALTLERNEN

(Referat)

Universitätsstudenten wurden in Hinsicht auf ihre Wiedererkennung und ihr Behalten einer Stelle von schwieriger Prosa durch zwei entgegengesetzte Areten von Tests untersucht: die eine erfordert die beinahe wörtliche Wiedergabe der Teile des Textes; die andere erfordert die Wiedererkennung des Sinnes, wenn der Text in ganz anderen Worten

ausgedrückt wird. Die Häufigkeit der Wiederholungen (von 1 bis 4 Durchlesen des Textes) beeinflusste die wörtlichen Testeinzelheiten, aber nicht die nichtwörtlichen. Die Wiedererkennung beider Arten von Testeinzelheiten war besser nach 24 Stunden als unmittelbar nachher, aber die wörtlichen Einzelheiten zeigten die Ebbinghaus-Vergessenkurve, weil die nichtwörtlichen Einzelheiten wahrscheinlich über Zeiträume bis zu 70 Tagen gar nicht vergessen wurden, und in einigen Fällen zeigten sie eine statistisch zuverlässige Zunahme. Diese Ergebnisse weisen darauf hin, dass das Inhaltlernen nicht nur von dem Lernen der sinnlosen Silben verschieden ist, sondern auch von dem sogenannten logischen Leren, bei dem die Wörter des ursprünglichen Textes behalten werden. Die Notwendigkeit eines selbständigen Studiums des Inhaltlernens wird dadurch angedeutet. Die Hypothese wird vorgeschlagen, dass dem Lernen eine enge und untrennbare Zusammenwirkung wenigstens zwei Vorgänge zukommt: der eine erscheint in dem wörtlichen Lernen und in der bedingten Aktion; der andere in "Einsicht," das Lernen von Inhalt.

ENGLISH, WELBORN, UND KILLIAN

THE DIFFERENTIATIVE EFFECTS OF AGE UPON HUMAN LEARNING*[1]

From the Department of Psychology of Stanford University

FLOYD L. RUCH

Certain *a priori* considerations, based for the most part on current neural theories of learning, on the one hand, and various biological theories of the nature of senescence, on the other, led the writer to formulate an hypothesis which is verified by the experiments to be reported here. Since learning, regardless of the nature of the neural phenomena underlying it, must involve changes either in the internal-cellular or anatomical organization of nervous tissue, and since the best of modern biological thought concurs in the statement that senescence must be regarded as a condition of lowered plasticity of tissue, the writer was led to formulate the following hypothesis: aged as compared with young learners should show a greater deficit in that learning which requires extensive re-organization of pre-existing habits than in that learning which makes use of previously-formed habits; while that learning which is neither interfered with nor aided by old habits should show an intermediate degree of deficit of the aged.

EXPERIMENTAL PROCEDURE

The description of the experimental procedure employed will be organized about five important considerations. These are: (1) manner of selecting cases and sizes and age of the populations; (2) control of motivation; (3) validity of the tasks; (4) general setting of the experiment; and (5) apparatus and specific learning procedure.

1. *Selection of Cases.* The selective factors which operated in obtaining the subject groups upon which this study was based are impossible to describe in quantitative terms. We must be content to give a verbal picture of the methods employed.

In searching for an adequate source of subjects, Professor Miles

*Accepted for publication by Walter Miles of the Editorial Board and received in the Editorial Office, March 13, 1934.

[1]This paper is a condensed report based on a Ph.D. Thesis accepted by the Graduate Faculty of Stanford University in 1930. The work was carried out under the direction of Professor Walter R. Miles as a part of a project made possible by a grant from the Carnegie Corporation.

hit upon the scheme of appealing to certain organized groups. Here, it was felt, *esprit de corps* would assist in obtaining cooperation. Accordingly, the Girl Scouts of Palo Alto were appealed to through their leaders. The following program was launched with this group: each scout who succeeded in obtaining the cooperation of one of her parents and one of that parent's parents would be accredited with a certain sum of money to be paid directly to the organization. The basis of payment was two cents per year of subject's age per two-hour session in the experiment. For instance, a person of sixty years would net the organization one dollar and twenty cents, etc. The scouts themselves were scheduled at seventy-five cents for each session, and a bonus of twenty-five cents for each male in the older group was given. When this procedure was expanded through appealing to other organizations such as church and lodge societies which could furnish subjects of ages comparable to those of child, parent, and grandparent, little difficulty was experienced in filling up the experimental groups with subjects who satisfied the following criteria: strength enough to come to the laboratory; ability to hear well enough for conversational purposes; ability to see well enough for ordinary reading; ability to speak English well.

The following table, Table 1, shows the amount of relationship between the members of the three age groups.

TABLE 1

AMOUNT OF RELATIONSHIP OF THE SUBJECTS

Relationship	Young *vs.* middle-aged		Young *vs.* old	Middle-aged *vs.* old	
	N	%	N	N	%	N
In a triad	3	7.5	3	7.5	3	7.5
In a diad	13	32.5	7	17.5	2	5.0
Unrelated	24	60.0	30	75.0	35	87.5
Total	40	100.0	40	100.0	40	100.0

The average ages and the age ranges of the three groups were determined largely by necessity. It was not possible to stipulate exactly the ages of the subjects. It was, however, possible to obtain age ranges adequate for the purpose of the experiment. Table 2 gives the age frequencies of the cases employed.

2. *Motivation.* Thorndike (5) states that adults might be expected to learn less well than children because most adults have already learned about as much as they have any need to know. Since

TABLE 2
DISTRIBUTION OF AGES

Young group		Middle-aged group		Old group	
Age	Frequency	Age	Frequency	Age	Frequency
17, 6-11	2	58-9	4	82-3	1
17, 0- 5	0	56-7	3	80-1	6
16, 6-11	3	54-5	5	78-9	3
16, 0- 5	3	52-3	8	76-7	0
15, 6-11	3	50-1	3	74-5	2
15, 0- 5	5	48-9	3	72-3	7
14, 6-11	5	46-7	1	70-1	4
14, 0- 5	4	44-5	4	68-9	4
13, 6-11	6	42-3	4	66-7	3
13, 0- 5	3	40-1	2	64-5	2
12, 6-11	2	38-9	2	62-3	7
12, 0- 5	4	36-7	0	60-1	1
		34-5	1		

additional learning has no great adaptive value for them, their motivational level is lower and their progress somewhat slower than that of younger individuals. The writer is able to verify this observation in the extreme instance of the inmates of two homes for the aged. Experiments preliminary to the present one were attempted at intervals throughout the years 1927 to 1929. Those attempts were distinctly lacking in promise because the inmates refused to work at the tasks set before them. They had apparently "let down" from any active struggle in life and were passively vegetating.

In the present study the problem of motivation did not prove to be so difficult as was at first anticipated. The subjects in all of the age groups, as has already been implied, represented a rather high socio-economic level. They were all sympathetic with the purpose of the experiment. Since no compulsion was employed, the individuals who finished the series of learning tasks were presumably well motivated. This is not astonishing when it is recalled that the older subjects were men and women whose lives had already been affected by advancing age. There appeared to be a certain solidarity within the older group which was expressed by one of them as a desire to prove that "old people are worth something after all."

Even though differences between the age groups in degree of motivation had existed, this would not have rendered invalid the conclusions of this experiment. The important comparisons involve differences in the performances of the same individuals on different

tasks and it is to be expected that motivational level would influence all tasks. If the obtained differences are caused by differential motivation for the various tasks, that is in itself significant.

3. *Validity of the Tasks.* The tasks which were included in the final battery represented the best approximation to the following criteria that it was possible to achieve after much preliminary experimentation and extensive searching of the literature.

(*a*) *The task must be short.* Had the tasks been too long fatigue might have entered to obscure the results. The two motor tasks required but twelve and a half minutes of working time for each with the remainder of the period given over to listening to directions and to resting. The three verbal tasks required but six minutes each of exposure time.

(*b*) *The task must be interesting.* The order of presentation of the five learning tasks was such as to give the best carry-over of interest. The attitude of competition in sport was suggested and most of the subjects seemed to catch that spirit.

(*c*) *The task must be learnable.* This criterion has a double implication. In the first place, people become discouraged when they fail to progress. In the second place, since the object of the experiment was to study rate of learning as a function of age and task, it is obvious that some learning must occur in all subjects.

(*d*) *The task must be uninfluenced by special training or vocational experience.* This criterion is difficult to meet in actual practice since no person has been reared in a psychological vacuum. As a working expedient, however, an attempt was made to employ tasks with which all individuals were equally familiar.

(*e*) *The task must be reliable as a psychological measure.* Statistical measures of the reliability of the tests will be presented below.

(*f*) *The tests must possess certain psychological characteristics.* Since the aim of the experiment was to relate the deficits of the aged as compared with the young learners to the degree of habit reorganization required by the various tasks, it is obvious that the whole structure of the problem depends upon the acceptance of the writer's analysis of the psychological characteristics of the tasks.

(*g*) *Each task must measure something that the others do not, i.e., the intercorrelations must be low.* The data bearing on this point will be presented below.

The tasks which were finally selected did not perfectly meet all of the above-listed criteria. This is not astonishing when we consider that certain of the criteria are mutually exclusive, as shortness *versus* reliability.

Five learning tasks were employed. Two of them were of the motor-learning type and three involved the learning of verbal materials. The first of the motor-learning tasks demanded the formation of a direct-vision pursuit habit. The second required the learning of a pursuit habit in mirror-vision. It was argued that the learning of the direct-vision habit involved the putting together of previously acquired partial-habit systems. Everybody has practiced direct-vision eye-hand coordinations in the numerous daily activities of picking up objects, writing, pointing, etc. When called upon to master a new and complex direct-vision eye-hand coordination the normal human subject has at his disposal many such simple habits. However, when called upon to master a complex eye-hand coordination guided by mirror-vision the previously formed direct-vision habits interfere. The three verbal learning tasks, presented by an exposure apparatus, included a series of simple paired associates, a series of nonsense equations, and a series of false multiplication products. In the case of the meaningful associates (example: HORSE SHEEP) the subject is able to call upon his past associations, while in the case of the nonsense equations this resource is lacking, and in the case of the false products previously-formed associations interfere.

4. *General Plan of the Experiment.* This experiment was part of a larger project described in detail elsewhere (2). The subjects who participated in the present experiment all devoted two two-hour periods spaced about two weeks apart to it. Each session was divided into four half-hour units. The first three units of the first session were given over to measurements performed by other members of the project. The last half hour of the first session and the entire time of the second session were given over to the present experiment. The direct-vision learning task came at the end of the first session while the second session was devoted to the four other tasks in the order: mirror-vision, meaningful associates, nonsense equations, false products. This order seemed to give the greatest carry-over of interest in terms of the entire project.

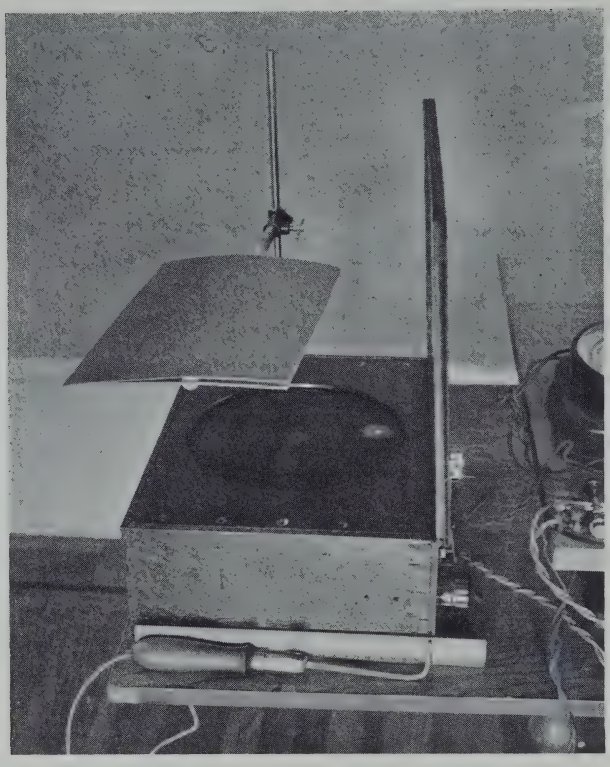

FIGURE 1
CLOSE-UP VIEW OF PURSUIT ROTOR SHOWING SCALE IN INCHES

5. *Description of Apparatus and Procedures.*

(*a*) *Direct-vision pursuit rotor.* Figure 1 shows a close-up view of the rotor with its screen in position. When the apparatus was used in direct-vision the screen was placed to one side and the mirror was covered with a piece of cardboard. This apparatus may briefly be described as a modified Koerth pursuit rotor (1A). The essential difference between it and that of Koerth was that in the present instance the circuit through the motor was closed only when the subject held the hand contact on the brass button of the rotor disk. With an apparatus of this sort the accuracy of the subject's pursuit movements is shown as the number of revolutions made by

the rotor in a constant period of time. An added feature is that the difficulty of the task increases as the subject's skill increases.

The subject sat in a comfortable chair at a convenient distance from the rotor which was placed so that the disk was about seventy centimeters from the floor.

It will be seen from a study of Figure 2 that the wiring of the

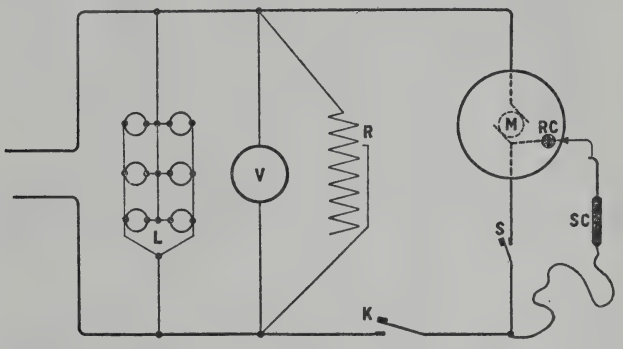

FIGURE 2

WIRING DIAGRAM OF PURSUIT ROTOR

L, bank of lamps.
R, variable resistance.
K, telegraph key for starting and stopping the trial.
S, switch for short-circuiting moving contacts when warming motor or checking speed.
M, electric motor (Bodine Electric Co.; Type CR-2).
SC, subject's contact.
RC, rotor disk contact.

apparatus was such as to provide for: accurate control of working time by the experimenter; control of the voltage across the terminals of the motor; facilities for warming up the motor and checking its speed.

It was soon discovered that the motor used in this apparatus showed a warming-up effect. Fortunately, it was possible to warm the apparatus up by continuous running with the contacts short circuited to a point beyond which further running did not affect its speed. Table 3 presents the results of a series of checks made at the ends of consecutive five-minute periods of running. It is seen from this table that the warming-up effect has about disappeared when the speed of 16.0 revolutions per thirty seconds is reached. The

TABLE 3

ROTOR SPEED-CHECKS BY FIVE-MINUTE INTERVALS OF CONTINUOUS RUNNING

Interval	Tenth revolutions per 30 seconds
Start	109
End 5 minutes	132
End 10 "	151
End 15 "	152
End 20 "	157
End 25 "	160
End 30 "	160
End 35 "	160
End 40 "	163
End 45 "	162
End 50 "	160

motor was always warmed up to this point before records were taken.

The following instructions were given.

> This is a test of eye-hand coordination. I shall explain the apparatus to you and then illustrate the procedure which you are to follow. This box contains an electric motor wired so that the circuit is closed when this contact is held upon this small brass button (point). The motor will turn only when the contact is held upon the button. To get the best results the contact should be held as near the center of the button as possible. The test procedure is as follows: (E takes the contact in his hand and carefully holds it in the standard position.) When I say "Ready!" place the tip of the contact on the button in this fashion. Upon the signal "Go!" I shall turn on the current, causing the disk to rotate as long as the pointer is held on the button. The object is to hold the contact in place as much as possible and thus cause the disk to turn as fast as you can make it do so. Your score is the number of revolutions you can give the disk during a working period of thirty seconds. At the end of this period I shall say "Rest!" at which time you are to lay the contact down and relax until I say "Ready!" whereupon you are to start the next trial. The rest periods will be twenty seconds long. We shall continue until twenty-five trials have been completed. You will be scored on the total number of revolutions which you can make in the twenty-five trials and also on your highest single score. Try to beat your previous record on every trial. I shall

tell you your score at the end of each trial. Do not practice during the rest periods. This test has been standardized for a particular manner of holding the contact (illustrate). It is especially important that you follow this standard in order that your scores may be compared with those of others. This short part should be held in a vertical position and the handle must be held as nearly horizontal as possible. The pointer was purposely made flexible so that you can not apply pressure but must depend entirely upon visual guidance.

"Do you understand?" E repeated until it was certain that S understood the directions. "Remember, to get a good score you must work your very best on every trial."

"Ready! Go! . . . . Rest!"

If S showed signs of fatigue or discouragement he was urged to keep on trying. The aim was to keep each learner working his hardest.

The score recorded was the total number of tenth-revolutions made in twenty-five periods of thirty seconds. It should be pointed out that scores of this type are influenced by the initial ability of the subjects as well as his rate of improvement. Data taken in this manner permit the calculation of coefficients of correlation between *odd-trial* and *even-trial* sub-total scores. Coefficients of this sort, although not reliability coefficients in the strictest sense of the term, do represent a sort of internal-consistency measure. These coefficients are listed in Table 4.

TABLE 4

DIRECT-VISION ROTOR CONSISTENCY COEFFICIENTS

Age group	N	Raw coefficient	Spearman-Brown
Young	40	.964±.008	.982
Middle-aged	40	.966±.008	.983
Old	40	.964±.008	.982

The intercorrelation of the direct-vision rotor with the other tests are given in Table 5. It is quite apparent from a study of the magnitude of these intercorrelations that the direct-vision rotor is measuring something relatively independent of the other tests. Moreover, when the effect of age on the correlation between MVR and DVR in the case of the old group is partialled out the coefficient becomes .471.

TABLE 5
INTERCORRELATIONS OF DIRECT-VISION ROTOR

Measure	Young group r	Young group P.E.	Middle-aged group r	Middle-aged group P.E.	Old group r	Old group P.E.
Age	.196	±.102	—.316	±.096	—.505	±.080
Intel.*	.062	±.106	.335	±.094	.281	±.098
MVR	.235	±.100	.533	±.076	.627	±.066
Assoc.	—.061	±.106	.227	±.102	.381	±.091
Nons.	.060	±.106	.253	±.099	.235	±.101
Intrf.	.140	±.105	.497	±.080	.297	±.097

*Correlations involving intelligence-test scores are based on 39 cases, others on 40 cases.

(*b*) *Mirrow-vision rotor.* This test employed the same apparatus as was used with direct-vision. The only change consisted in removing the shade from the mirror and in placing a screen so as to obstruct direct vision without interfering with the arms and hands of the subjects.

Following are the instructions. E removed the shade, adjusted the screen and said:

> You will remember this apparatus. Today you are to work in the same fashion that you did before, except that the disk will be seen in the mirror. (As much of the instructions were repeated as seemed necessary to insure that S understood.) Of course, this experiment is much harder than the other one. Consequently you cannot expect to make as high a score. Don't get discouraged; work as hard as you can; you will be surprised at how well you can do after a little practice.

The rest of the procedure was similar to that of the direct-vision rotor.

Scores were kept as before. Consequently, these data permit the same type of treatment and are subject to the same limitations that were indicated in the discussion of DVR scores. Table 6 gives the consistency coefficients based on odd *vs.* even trials.

TABLE 6
MIRROR-VISION ROTOR CONSISTENCY COEFFICIENTS

Age group	N	Raw coefficient	Spearman-Brown
Young	40	.965±.008	.982
Middle-aged	40	.964±.008	.982
Old	40	.827±.032	.905

The intercorrelations of the mirror-vision rotor with the other scores tend to run about the same as those for the direct-vision rotor, a fact which may be seen by inspection of Table 7.

TABLE 7
INTERCORRELATIONS OF MIRROR-VISION ROTOR

Measure	Young group r	P.E.	Middle-aged group r	P.E.	Old group r	P.E.
Age	.350	±.094	—.460	±.084	—.592	±.068
Intel.	.344	±.094	.357	±.093	.496	±.080
DVR	.235	±.100	.533	±.076	.627	±.066
Assoc.	—.234	±.101	.159	±.104	.366	±.093
Nons.	.084	±.106	.097	±.106	.356	±.093
Intrf.	.085	±.106	.330	±.095	.292	±.097

It is seen from this table that the highest intercorrelation, .627, is found in the case of mirror-vision rotor *vs.* direct-vision rotor. When age is partialled out this becomes .471, as has already been seen. This indicates that, while the two tests have a great deal in common other than the effect of age, they are not measuring the same thing. It is interesting to note that the correlation of the mirror-vision rotor with age is consistently greater than the similar correlation of the direct-vision rotor. It should be further noted that the two older groups show a higher negative correlation between age and mirror-vision rotor than they do between age and direct-vision rotor.

(*c*) *Paired associates.* The apparatus and general procedure were the same in all of the verbal-learning tests. The apparatus consisted of an exposure device which, by means of a series of electromagnets activated by a timer, dropped cards of suitable size bearing the learning materials. After some preliminary trials it was decided that the following exposure times gave the best results. The associate on the left was first exposed five seconds. This gave the subject ample time to read it and try to think of its mate. Then the entire contents of the card were exposed for a period of one second. This time was sufficient for the subject to read the second member, but was short enough to insure his sustained attention. After the card had dropped out of sight an interval of one and a half seconds elapsed before the second card was released. The apparatus was activated automatically by a simple timing device which need not be described here.

The exposure cards were ten by fifteen centimeters in size cut from highly durable cardboard. The lettering was done with Wilson's black gummed letters seven millimeters high.

The items employed in this test follow. An attempt was made to select items which involved only very familiar words.

MAN	BOY (Sample)
SOFT	CHAIR
WALK	CAR
TREE	FLAG
HOUSE	VISIT
ROOM	LIGHT
STEM	BUD
NEST	OWL
WIND	BROOK
HORSE	SHEEP
WHITE	PINK

The following instructions were given:

> See this card. This is a sample of the type of material you are to memorize. It is only a sample and will not be included in the regular series. Ten cards, each containing a pair of related words, will be presented to you automatically by this apparatus. When the card drops, revealing the word to the left, you are to read it aloud. After five seconds it will drop to another position showing the word to the right. Read this word aloud also. As you read the two words try to associate them in your mind in such a fashion that the next time you see and read the word to the left it will suggest its mate. Ten cards will be presented to you in this fashion. Then the cards will be shuffled and sent through again. When you see the key word to the left, read it aloud and immediately after this pronounce the word that goes with it. If you can give the second word before it drops into sight you get one point credit. If you cannot recall the word, simply wait until it appears, then read it aloud, striving to remember it for another time. Your score will be the number of words correctly anticipated during the fifteen presentations of the ten pairs.
>
> (Illustrate the procedure with the sample card until it is apparent that S comprehends the procedure.)
>
> "Trial one. Ready!"
>
> At the end of each trial the experimenter said: "Now you will try to give the second word before it appears in view. If you are not sure, guess. An incorrect guess does not count against you any more than remaining silent.
>
> "Trial two. Ready!"
>
> (At the end of each trial the subject was told his score.)

As may be seen from the instructions, the score employed was the total number of items correctly anticipated during fifteen presentations of the ten cards. Since the subjects could not possibly have anticipated on the first trial, 140 was the highest score possible. Unfortunately this test proved a little too easy for the younger group, as was shown by the fact that one individual earned the maximum score. The distribution for the younger group shows considerable piling up at the higher end. Any possible bearing that this might have on the interpretation of the scores will be discussed later. The test was not too difficult, as is seen from the fact that the lowest score earned is 41. These scores more nearly represent "true learning ability" than do those of the rotor tests since here all subjects start with zero knowledge of the associates. Of course, one group may show an initial superiority over another in general habits of work or methods of attack.

Consistency coefficients for the three age groups were determined by ranking the items on the basis of difficulty within a particular age group and then dividing the items so ranked into two forms of equal difficulty. The correlations based on the split-form scores are given in Table 8.

TABLE 8

CONSISTENCY COEFFICIENTS FOR PAIRED ASSOCIATES

Age	N	Raw coefficient	Spearman-Brown
Young	40	.686±.057	.814
Middle-aged	40	.538±.076	.700
Old	40	.801±.038	.890

The intercorrelations of the paired associates with the other tasks are given in Table 9.

TABLE 9

INTERCORRELATIONS OF PAIRED ASSOCIATES

Measure	Young group r	Young group P.E.	Middle-aged group r	Middle-aged group P.E.	Old group r	Old group P.E.
Age	.038	±.107	—.225	±.101	—.387	±.091
Intel.	.031	±.107	.131	±.105	.698	±.054
DVR	—.061	±.106	.227	±.102	.381	±.091
MVR	—.234	±.100	.159	±.104	.366	±.093
Nons.	.354	±.093	.219	±.102	.693	±.054
Intrf.	.262	±.099	—.080	±.106	.594	±.068

(*d*) *Nonsense materials.* The apparatus and procedure employed in this variant of the experiment were essentially the same as those used with the paired associates. The following nonsense items were presented:

E × Z = G (sample.)
A × M = B
N × M = C
L × B = D
S × Q = H
H × Y = J
B × D = M
R × S = Q
W × L = S
K × U = T
F × P = V

The instructions were the same as before except that it was pointed out that the nonsense material would be more difficult to memorize than the paired associates and that a smaller number of correct anticipations would still be a good score. The score was the total number of correct anticipations in fifteen showings of the ten pairs. The consistency coefficients, computed in the same way as in the case of the paired associates, are given in Table 10.

TABLE 10
CONSISTENCY COEFFICIENTS FOR NONSENSE LEARNING

Age group	N	Raw coefficient	Spearman-Brown
Young	40	.539±.076	.700
Middle-aged	40	.737±.048	.848
Old	40	.804±.038	.891

The intercorrelations are given in Table 11.

TABLE 11
INTERCORRELATIONS OF NONSENSE LEARNING

Measure	Young group *r*	Young group P.E.	Middle-aged group *r*	Middle-aged group P.E.	Old group *r*	Old group P.E.
Age	—.085	±.106	—.224	±.101	—.251	±.100
Intel.	.339	±.094	.350	±.093	.713	±.051
DVR	.061	±.106	.253	±.009	.235	±.101
MVR	.084	±.106	.097	±.106	.356	±.093
Assoc.	.354	±.093	.219	±.102	.693	±.054
Intrf.	.404	±.090	.473	±.080	.833	±.031

(*e*) *Interference learning.* The following false multiplication products were presented with the technique used with the other verbal materials:

3 × 5 = 25 (sample.)
3 × 1 = 1
3 × 4 = 2
1 × 1 = 3
3 × 3 = 4
6 × 3 = 5
5 × 1 = 7
2 × 5 = 8
2 × 4 = 9
5 × 5 = 11
5 × 4 = 14

The score recorded was the number of correct anticipations in the series of fifteen exposures of the ten cards. Tables 12 and 13 give the consistency coefficients and intercorrelations of this task.

TABLE 12
CONSISTENCY COEFFICIENTS FOR INTERFERENCE LEARNING

Age group	N	Raw coefficient	Spearman-Brown
Young	40	.635±.063	.777
Middle-aged	40	.714±.051	.833
Old	40	.663±.060	.797

TABLE 13
INTERCORRELATIONS OF INTERFERENCE LEARNING

Measure	Young group *r*	Young group P.E.	Middle-aged group *r*	Middle-aged group P.E.	Old group *r*	Old group P.E.
Age	.175	±.103	—.265	±.099	—.206	±.102
Intel.	.115	±.105	.402	±.089	.555	±.073
DVR	.140	±.105	.497	±.080	.297	±.097
MVR	.085	±.106	.330	±.095	.292	±.097
Assoc.	.262	±.099	—.080	±.106	.594	±.068
Nons.	.404	±.090	.473	±.082	.833	±.031

METHODS OF TREATING THE DATA

The major conclusion of this study is that old age in man brings a deterioration of learning ability which is not general but depends to a significant extent upon the nature of the material learned. The present section will be devoted to the proof of this conclusion.

In attempting to make comparisons between individuals or groups

of individuals the psychologist is handicapped by the arbitrary nature of the units of his measuring devices. There is rarely an acceptable demonstration that the differences on one part of his scale are equivalent to the same numerical differences on another part. Nevertheless, the only statistical procedures available for the treatment of the data of this experiment assume that the arbitrary units employed do not systematically differ from true units. More important is the fact that the absolute zero point of ability is usually unknown. There are, however, certain methods which make no assumption with regard to the true zero point. Such a method has been employed in treating the data of this experiment.

1. *Critical Ratios as Measures of Differential Deficit.* The deficit of a particular age group as compared with another is defined as the ratio of the difference between the means of the two groups to the standard error of that difference. This measure, commonly referred to as a critical ratio, expresses the probability of an observed difference being due to something other than chance. By determining the critical ratios for each of the five learning tasks employed in this study it was possible to rank the tasks in the order in which they showed a deficit of one age group as compared with another.

The formula for calculating the standard error of a difference between two means follows:

$$\sigma_{M_1 - M_2} = \sqrt{\sigma_{M_1}^2 + \sigma_{M_2}^2 - (2r_{M_1 M_2}\, \sigma_{M_1}\, \sigma_{M_2})}$$

When the means are not correlated the term in the parentheses drops out. Since certain of the subjects employed in the present experiment were biologically related, it was necessary to correct for the slight correlation of means thus produced. Table 14 gives the coefficients of correlation showing the amount of relationship be-

TABLE 14

CORRELATION OF AGE GROUPS

Task	Young *vs.* Old		Young *vs.* Middle		Middle *vs.* Old	
	Related	Total	Related	Total	Related	Total
DVR	—.0037	—.0009	.0732	.0293	.5805	.0726
MVR	—.0038	—.0010	.0861	.0344	.0915	.0114
Assoc.	.2510	.0625	.5440	.2176	—.7557	—.0945
Nons.	—.1400	—.0350	.3750	.1500	—.5437	—.0680
Intrf.	.0020	.0005	.4010	.1604	.8276	.1035
N	10	40	16	40	5	40

TABLE 15

AGE-DEFICIT CALCULATIONS

	Young *vs.* Old								
	M_y	M_o	SD_y	SD_o	SD_{M_y}	SD_{M_o}	$D_{M_y-M_o}$	SD_{D_M}	C
DVR	2857	2392	244.3	415.9	38.6	65.7	465	76.23	6.100
MVR	771.9	406.2	214.2	166.1	33.8	26.2	365.7	42.79	8.546
Assoc.	134.7	111.6	4.26	26.79	.67	4.23	23.1	4.241	5.447
Nons.	78.5	37.9	23.72	25.62	3.75	4.05	40.6	5.615	7.229
Intrf.	106.1	49.4	19.45	24.07	3.07	3.80	56.7	4.884	11.609
	Middle-Aged *vs.* Old								
	M_m	M_o	SD_m	SD_o	SD_{M_m}	SD_{M_o}	$D_{M_y-M_o}$	SD_{D_M}	C
DVR	2805	2392	287.2	415.9	45.4	65.7	413	77.10	5.357
MVR	740.0	406.2	286.2	166.1	45.2	26.2	333.8	51.98	6.422
Asoc.	123.7	111.6	18.28	26.79	2.89	4.23	12.1	5.344	2.264
Nons.	62.8	37.9	27.95	25.62	4.42	4.05	24.9	6.195	4.019
Intrf.	76.1	49.4	29.00	24.07	4.58	3.80	26.7	5.640	4.734
	Young *vs.* Middle-Aged								
	M_y	M_m	SD_y	SD_m	SD_{M_y}	SD_{M_m}	D_{y-M_m}	SD_{D_M}	C
DVR	2857	2805	244.3	287.2	38.6	45.4	52	59.02	.881
MVR	771.9	740.0	214.2	286.2	33.8	45.2	31.9	55.50	.575
Assoc.	134.7	123.7	4.26	18.28	0.67	2.89	11.0	2.821	3.899
Nons.	78.5	62.8	23.72	27.95	3.75	4.42	15.7	5.350	2.934
Intrf.	106.1	76.1	19.45	29.00	3.07	4.58	30.0	5.088	5.896

tween the performances of the relatives. These coefficients, corrected for the attenuating effect of unrelated individuals, are also included.

Table 15 gives the critical ratios expressing the deficit of each age group as compared with the two others for the five tasks investigated. The various measures involved in the calculation of these ratios are also included in this table. It is seen that for any age comparison the tasks may be ranked in the order of their difficulty for the older as compared with the younger learners. Take, for example, the comparison of the young and old groups on the five tasks. It is seen from this table that the young group was superior to the old group on all of the tasks. This, however, is an incidental point. The significant fact is that the degree of superiority of the younger group varied with the task. The younger group excelled the older group more in the mirror-vision than in the direct-vision rotor learning. Stated conversely, this means that the deficit of the aged group was greater for the mirror-vision than for the direct-vision rotor. The same type of interpretation may be applied to the three critical ratios based on the comparison between the young and old groups on paired associates, nonsense, and interference scores. The old subjects as compared with the young had the least difficulty in mastering the paired associates, experienced greater difficulty in mastering the nonsense material, and showed the greatest deficit for the interference material. The same type of treatment is applicable to the other two age-group comparisons. There is, however, still one more step in the quantitative expression of deficit as a function of the type of learning task. This next step yields a measure of the reliability of the ranking of tasks in terms of the deficit of the aged.

Like all statistical measures, the critical ratio has an error, and consequently differences between critical ratios must be expressed in relation to the standard errors of their differences. The formula employed in this study for determining the significance of a difference between two critical ratios follows:

$$\sigma_{c_1-c_2} = \sqrt{\sigma_{c_1}^2 + \sigma_{c_2}^2 - (\text{a correlation factor})}$$

It will be observed that this formula should contain a correction for correlational effects. Careful consideration, however, has indi-

cated that these correlational effects would be small and positive. That is, if the correlational correction were employed the end result would be to increase rather than decrease the differences between the critical ratios. Upon turning our attention to the remaining terms of the formula, it should be noted that the standard error of a critical ratio is unity by definition. That is, critical ratios due to chance alone show a unitary deviation around a mean of zero. Consequently the standard error of the difference between two critical ratios becomes 1.4142.

$$\sigma_{c_1} = 1$$
$$\sigma_{c_2} = 1$$
$$\sigma_{c_1 - c_2} = \sqrt{1^2 + 1^2} = \sqrt{2} = 1.4142$$

To determine the significance of the ranking of the five tasks in terms of the critical ratios of the mean differences for the tasks, it is necessary merely to compute the difference in the magnitude of the two compared ratios and divide this difference by 1.4142. Table 16 shows comparisons of this type for the young *vs.* old groups.

TABLE 16

SIGNIFICANCE OF THE DIFFERENTIAL DEFICITS BETWEEN YOUNG AND OLD GROUPS

Tasks	C-C	C-C/SE
DVR *vs.* MVR	2.446	1.73
Assoc. *vs.* Nons.	1.782	1.26
Nons. *vs.* Intrf.	4.380	3.10
Intrf. *vs.* Assoc.	6.162	4.36

Since these two groups show the greatest average age differences the results from these comparisons are the fundamental ones.

From inspection of Table 16 it is seen that the rank order of the tasks on the basis of increasing deficit of the aged as compared with the young subjects possesses a fair degree of reliability. It should be recalled that these differences would be somewhat greater if the correlational correction had been applied, and, more important, the order of deficit is consistent with that expected on the basis of the hypothesis stated in a previous section.

The results based on the comparison of the middle-aged and old groups are consistent with those for the young as compared with the old group. Since the average age difference between the middle-aged and old groups was less than that between the young and

old groups, one would expect to find less marked deficits. Table 17 shows that this was, indeed, the case. The significant fact is, however, that the same order of deficits was found.

TABLE 17

SIGNIFICANCE OF THE DIFFERENTIAL DEFICITS BETWEEN THE MIDDLE-AGED AND OLD GROUPS

Tasks	C-C	C-C/SE
DVR *vs.* MVR	1.065	0.75
Assoc. *vs.* Nons.	1.755	1.24
Nons. *vs.* Intrf.	0.717	0.51
Assoc. *vs.* Intrf.	2.470	1.75

Tables 15, 16, and 17 indicate that the older learners showed the same order of deficits as a function of the task when their scores were compared with those of the middle-aged group as when their scores were compared with those of the young group, i.e., they found the mirror-vision rotor harder than the direct-vision rotor; nonsense materials harder than paired associates; interference materials harder than nonsense materials; and, obviously, interference materials harder than paired associates.

When the young group was compared in like manner with the middle-aged group of learners the trend of the data was not so clear. Table 18 gives the significance of the differences in age deficits for the five learning tasks.

TABLE 18

SIGNIFICANCE OF THE DIFFERENTIAL DEFICITS BETWEEN YOUNG AND MIDDLE-AGED GROUPS

Tasks	C-C	C-C/SE
DVR *vs. MVR*	—0.306	—0.22
Assoc. *vs.* Nons.	—0.965	—0.68
Nons. *vs.* Intrf.	2.962	2.09
Assoc. *vs.* Intrf.	1.997	1.41

It is seen that no large difference in the deficits of the two age groups existed for mirror-vision rotor *vs.* direct-vision rotor or for paired associates *vs.* nonsense material. Furthermore, the direction of these deficits was reversed. That is, the middle-aged subjects in so far as the difference is statistically significant did relatively better on the mirror-vision rotor than on the direct-vision rotor, and they also did relatively better on nonsense materials than on paired

associates. The reason for this reversal probably lies in the fact that the points of prime in the growth curves for the abilities concerned come at different ages. It should also be noted that the generalized curve of growth of mental abilities with age is such that the young and middle-aged groups are located fairly near the same level, the younger subjects being grouped at ages just below the points of prime and the middle-aged subjects being grouped just beyond.

In summarizing the results of this investigation it is stated that the old as compared with the young learners showed a greater deficit for the mirror-vision learning than they did for the direct-vision learning. With the three types of verbal learning the following order of deficit of the aged was found: paired associates; nonsense materials; interference materials. These differences in degree of deficit are statistically significant. The performances of the middle-aged as compared with the old group showed the same order of deficits of the ages as was seen in the young *vs.* old group comparisons, though to a lesser degree. Significant differences in the deficits of the middle-aged as compared with the young group were not found for the two motor-learning tasks nor for nonsense materials *vs.* meaningful associates; while for the other tasks the trend of the deficits was in the expected direction, i.e., the middle-aged subjects had greater relative difficulty with interference materials than with nonsense equations and greater relative difficulty with interference materials than with meaningful associates.

It is worth while to consider the possibility of the results of this experiment being due to some artificial or extrinsic group of causes rather than arising out of fundamental differences between the groups investigated. This type of consideration demands at the outset a careful definition of the constant and variable factors involved in the performance measured. In the present experiment the objective conditions were kept constant for all age groups in so far as possible. The learning tasks, distribution of work and rest periods, rate of exposure, place of work, experimenter, etc., were kept absolutely constant while certain other physical conditions such as time of day, temperature, humidity, though not held constant, did not vary systematically to favor a particular age group.

There were, however, certain variables which could not be readily controlled in the present experiment. It is well known that older people tire more easily than do younger ones. It is possible that

the factor of fatigue might have entered into the present experiment although an attempt was made to avoid this by employing short tasks with frequent rest periods. The direct-vision learning came at the end of the first two-hour session, while the mirror-vision learning came at the beginning of the second session. Fatigue when present to a greater degree in the aged than in the young learners would, therefore, tend to reduce the magnitude of the observed differences. However, in the case of the three verbal tasks, the order in which the tasks showed a deficit of the aged coincided with the order in which the tasks were performed, and hence with the order in which a progressive impairment from fatigue might be expected.

Another factor which must be considered is the possibility of a progressive loss of interest. The writer's observations in this regard indicate that the older subjects maintained their interest even better than the younger ones did. The younger subjects usually started with considerable enthusiasm only to lose some of it as the experimental period progressed.

Still another factor, and one which is difficult to appraise, is that of progressive adaptation to the test situation. This influence, if present, would tend to decrease the differences observed in the case of motor learning and verbal learning as well. A difference in rate of adaptation to the experimental situation between any two of the age groups would of course introduce an element of distortion.

It is well known that older individuals have poorer vision than do younger ones. This factor, however, would be a constant for all compared tasks and could not account for the observed order of deficits. It should be noted in this connection that older people have slower reaction times than do younger ones. Such a difference could not account for the observed order of deficits since exposure time was constant for all materials. In similar fashion it should be noted that age decreases the steadiness of movements as well as their speed. A decrease with age in speed and steadiness, however, could not account for the greater deficit of the aged in mirror-vision learning than in direct-vision learning since the former requires less steadiness and speed of movement than the latter.

In reviewing the discussion of the possibility of disrupting factors having been present in the experiments just reported, it appears that no systematic summation of those factors was present. Therefore, the conclusion that old age brings a differential deficit in learning

performance as a function of the nature of the learning situation seems to be the most tenable explanation of the observed facts of this experiment.

It is interesting to view the results of the present experiment in relation to the findings of previous investigations. Although there has been no previous attempt directly to study the differentiative effect of age on human learning ability, certain investigators have incidentally discovered facts bearing on this interesting problem (3). Willoughby (6) found greater deficit in a test involving learning of nonsense materials than he did in tests involving greater use of previously-acquired associations. This finding is consistent with the results of the present study. Jones, Conrad and Horn (1) found a greater decline with age in immediate logical memory than in Army Alpha scores. Army Alpha contains a large number of items which call on previously acquired associations. Thorndike (5) has presented data which agree with the results of this study with regard to the comparisons between meaningful and nonsense materials. Snoddy (4) has elaborated a theory to explain the differences between the form of the learning curves of young *vs.* old learners in the case of mirror drawing. His theory is consistent with the data obtained in the present investigation.

Summary and Conclusions

The learning abilities of three groups of forty individuals aged twelve to seventeen years, thirty-four to fifty-nine years, and sixty to eighty-two years have been studied. The subjects in the three age groups were comparable with regard to original ability. Moreover, the methods of treating the data were such that the major conclusions of this thesis do not require a demonstration of complete equality of the age groups in original ability.

Two motor tasks were employed. One of these, direct-vision rotor, required the use of partial-habit systems acquired by the subject in the normal course of his daily activities prior to the experiment. The second, mirror-vision rotor, differed significantly from the first in that it required the breaking down of old habits and the building up of new ones.

Three types of verbal learning were employed. The first, paired associates, involved the use of old associations between logically connected words. The second, nonsense equations, required the forma-

tion of new verbal habits and represented what Thorndike has called sheer modifiability. The third, false products, was employed as a type of material in which mastery is achieved at the expense of old associations.

The intercorrelations of the tests were satisfactorily low.

The objective conditions of the experiment, methods of scoring, and motivation of the subjects, were reasonably constant for all of the three age groups studied.

The average performances of the three age groups have been stated in terms of the degree of deficit of the older learners as a function of the type of learning material. Of the two motor tasks the mirror-vision variant showed the greater deficit of the aged as compared with the younger learner. With the verbal learning tasks the amount of deficit of the aged increased in the following order: paired associates, nonsense materials, false products. The magnitudes of the differences in amount of deficit as a function of the type of material were found to be statistically significant. When the middle-aged and old groups were compared the same order of deficits as a function of the nature of the tasks appeared, though it was less significant statistically. When the young and middle-aged groups were compared it was found that the direction of the deficit sometimes favored one group and sometimes the other. This situation arises, possibly, out of the fact that the young group included individuals who had not yet reached their highest ability while the middle-aged group contained individuals who had barely passed their peak of learning ability, on the one hand, and the further fact that the different abilities measured reach their peaks at different chronological ages, on the other.

From the biological concept of senescence as a condition of lowered plasticity of tissue and the further assumption that learning involves change in nervous tissue, it was hypothesized that old age should bring a differential deficit in learning ability as a function of the amount of re-organization of pre-existing habit patterns required in the learning. That is, since older people possess highly stable nervous tissues they should encounter greater difficulty in learning tasks whose mastery demands a greater amount of re-organization of previously formed habit patterns than in learning tasks whose mastery demands less of this re-organization. It is significant for psychological theory that the order of the tasks ranked according to increasing degree of

deficit of the aged as compared with the young or middle-aged learners, predicted on the basis of the considerations stated above, coincided exactly with the order of deficit found in this experiment.

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Stanford University
California

LES EFFETS DIFFÉRENTIATIFS DE L'ÂGE SUR LA CAPACITÉ D'APPRENDRE CHEZ LES HUMAINS

(Résumé)

Selon les théories courantes de la neurologie de l'apprentissage, de l'un côté, et la conception biologique de la sénescence comme une perte de la plasticité du tissu vivant, de l'autre côté, l'auteur a fait l'hypothèse que les vieux individus devraient montrer des pertes de la capacité d'apprendre quand on les compare aux plus jeunes et que ces pertes devraient varier avec la nature de la tâche à apprendre. Les tâches qui exigent un minimum de la réorganisation de vieilles habitudes devraient être moins difficiles pour le vieux qui apprend que les tâches qui exigent une quantité considérable de réorganisation des habitudes. Quelques expériences qui ont employé deux types de l'apprentissage moteur et trois types de l'apprentissage verbal avec 120 sujets variant entre l'âge de douze et celui de quatre-vingt-deux ans ont donné des resultats qui soutiennent cette hypothèse.

Ruch

DIE UNTERSCHIEDENDEN WIRKUNGEN DES ALTERS AUF DIE MENSCHLICHE LERNFÄHIGKEIT

(Referat)

Auf Grund der gegenwärtigen Theorien der Neurologie des Lernens einerseits, und der biologischen Auffassung des Alterns als ein Verlust der Plastizität der lebenden Gewebe andererseits, hat der Autor die Annahme aufgestellt, dass alte Individuen Mängel der Lernfähigkeit aufzeigen sollten,

wenn sie mit jüngeren Individuen verglichen werden, und dass diese Mängel mit der Natur der Lernaufgabe variieren sollten. Aufgaben, die ein Minimum von Reorganisation der alten Gewohnheiten erfordern, sollten dem alternden Lerner weniger Schwierigkeit geben als Aufgaben, die eine beträchtliche Reorganisation der Gewohnheiten verlangen. Experimente, welche zwei Arten von Motorlernen und drei Arten von Verballernen mit 120 Vpn. im Alter von zwölf bis zu zweiundachtzig Jahren anwenden, lieferten Resultate, die diese Annahme unterstützten.

RUCH

EYE-MOVEMENTS CORRELATED WITH INNERVATION OF THE ORBICULARIS OCULI*

From the Institute of Human Relations, Yale University

S. M. Newhall and H. M. Halverson

The original experiments of Dodge and the work of his followers in the field of eye-movements have so convincingly demonstrated their scientific, diagnostic, and educational importance that efforts to discover or establish new facts need no justification now. Still, a hitherto neglected technical problem which motivated the present study may be mentioned in its own right. In visual experiments which require relatively prolonged and strictly continuous retinal stimulation, movements of either the eyeball or the eyelid will prove disturbing to the extent that they result in cutting off or displacing the stimulus. Previous observations have shown that these sensory effects are neither infrequent nor trivial (4). A device was developed which would restrain the lid and so prevent occlusion of the pupil during the blink, provided the eye remained in the normal position. If the eye rolled out of the normal position at the time of the restrained blink the retinal stimulus would be reduced and displaced. Since the available data seemed inadequate, the technical problem was to measure the eye-movement, if any, at the time of lid-movement.

According to the early observations of Sir Charles Bell and of Albrecht von Graefe, and the recent experiment of Miles, the eyeball regularly rolls upward as the lid closes (3). The latter investigator[1] photographically recorded eye- and lid-movements by means of light reflected from the cornea and from small mirrors attached to the lids. "The subject fixated a certain point and periodically felt the natural corneal urge to wink. At other times he was asked to keep his eye on the fixation point and wink rapidly." The vertical displacement of the cornea was calculated from his

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[1]The literature is cited and the little-recognized contributions of Bell and v. Graefe are quoted at length.

records to be from 10° to 15°, the upward roll being diagonally nasalward instead of strictly vertical.

Experimental Questions

The data presented herewith bear upon two experimental problems. First, the direction and the amount of eyeball roll when a reflexive innervation occurs during normal fixation. The subject was asked to fixate a point in front of him, his eye was prevented from closing by a lid-restraining bar of the type elsewhere described, and at the moment of any reflex lid-movement, a ball-displacement might occur. Reflex lid-movements due to sudden or intensive external stimuli were not investigated. The second experimental question is that of the direction and degree of eyeball roll when a voluntary wink occurs during normal fixation.[2] This situation is the same as the other with the exception that the orbicularis innervation is voluntary instead of reflex. The question of eyeball roll when an unrestrained, free blink occurs is also being studied, but the data are neither sufficiently consistent nor abundant to interpret at this time. Here we encounter Miles's difficulty that often the closing lid interferes with the record of the ball-movement. Such data as we now have on the point, however, suggest that there may be no greater roll of the eyeball when the lid is unrestrained than when it is restrained.

Technique

The following cooperated as subjects throughout the study: Mrs. Helen Garvey, Dr. C. R. Garvey, Dr. S. B. Lindley, Dr. Helen Peak, Mr. Thomas Rolley, Dr. G. R. Wendt.

Lid-movements and eye-movements were recorded on a 16 mm. motion film at the speed of 16 exposures.[3] The subject, wearing a lid-holder on his dominant eye, was seated with his chin resting on a pad at the edge of a table and asked to fixate a marked point on the camera box two inches to the right of the lens. The eyes were on a level with the lens and by means of reference rods the distance between eyes and lens was carefully maintained. The camera was

[2]The term "wink" may be used to designate voluntary lid-closures while "blink" is used for the reflexive movements.

[3]A Bell and Howell Motion Camera, Filmo 70D, the speed of which we have accurately determined for continuous performance of 40 seconds, served as the photographing device.

run continuously for 15 seconds at a time, recording lid- and eye-movements simultaneously.

The film was studied by projecting upon a screen an image of the eye magnified to six diameters.[4] This magnification was used because under the conditions it seemed to permit maximum precision of measurement. With greater magnification the definition would be so reduced as to more than offset what would otherwise be a smaller absolute error of measurement. A large sheet of graph paper ruled in square mm. served as the projection surface. The magnified eye-movements and lid-separations were first measured in terms of mm. on this screen and later reduced to actual magnitudes; the eye-displacements were converted into degrees of angular movement.

The first step in viewing the film was to project it in motion for the purpose of locating the several blinks and winks recorded on it. Then, in case of each blink or wink, the frame containing the image of maximal lid-closure was marked and 32 frames, 15 of which immediately preceded and 16 of which immediately followed the marked frame, were reviewed in order by successive stilling of the individual frames. Thirty-two frames were examined because that number was sufficient to furnish a relatively long baseline of reference preceding the beginning and succeeding the ending of the given lid-movement.

A simple method was adopted for taking the measurements from the magnified image of the subject's eyes. The amount of lid-closure in a given blink or wink was estimated from the unrestrained eyelid. The vertical distance between the upper and lower lids was reported in terms of the mm. rulings on the projection screen. The reduced distance between the lids in the few frames in which the lid-movement occurred could then be compared with the approximately constant distance between the lids in the other frames when the eye was wide open.

The vertical and horizontal components of the eyeball movements during the blink or wink were estimated in somewhat similar fashion, but from the eye the lid of which was restrained. To secure the vertical component we recorded for each frame, first, the position of a reference dot on the lid-holder device above the eye and second, the position of the lower edge of the pupil. Since the first of these

[4]The film was projected by a Model C Kodascope (Eastman) with take-up pulleys which allow movement of the film in both the forward and backward directions and in addition permit "stilling" frame by frame.

two marks was fixed with respect to the subject's forehead, change in the distance between them would indicate movement of the eyeball. If the eye rotated downward the distance between the points would increase; if it rolled upward the distance would diminish. Thus a measure of the vertical displacement during the period of a blink or wink could be secured. Horizontal displacements of the eye were measured in similar manner, only in this instance the points of reference were a dot on the bridge of the lid-holder device and the near edge of the iris.

Precision of Measurements

Though the image on the projection screen was but six times the diameter of the subject's eye, the magnification of the picture in the film was of course relatively high. Therefore, when the projected image was observed from a distance of one or two feet, as in making measurements, it was not surprising to find that the reference marks were definitely fuzzy in outline.

So the task of investigator N was not merely to report the ordinate or abscissa upon which a clearly defined reference point or sharp line of demarcation fell, but rather to estimate where, in a narrow, graduated range of fuzziness or blur, the most representative coordinate lay. Practice made evident the fact that in this situation quick judgment was easier and more reliable than deliberative judgment. In some instances when the observer hesitated he found that it was necessary to look away from the projection screen and then try again before he could pass judgment. At first, as in many psychophysical problems, the observer's feelings of confidence or certainty were low and he did not feel satisfied with his individual judgments. But the task became considerably automatized and the judgments usually came quite rapidly and easily.

Inspection of the readings, which were recorded by investigator H, suggested surprising consistency. Still, in view of the rather small eyeball movements to be expected, it seemed desirable to make a preliminary check upon the reliability of the measurements. Accordingly, investigator N repeated his own readings from a section of film without any knowledge that he was doing so or that he would be called upon to do so. Even had he known that the same section of film was being run over, that knowledge could scarcely have resulted in increased consistency. In the process of successive stilling H frequently

shifted the position of the projected image and thereby the numerical values of the coordinates defining its position.[5]

Though there are only sixteen cases in each of the series compared, the standard errors of the means are 0.26° and 0.24°. The standard error of the difference is 0.36° while the critical ratio is 1.5. This is well within the random sampling range and suggests reasonable constancy of judgment tendency. Of course this check is too limited to establish a level of precision for the experimental judgments as a whole; but when it is considered in relation to the relatively consistent trends manifest in the experimental results, the indication is that a fair degree of precision was attained.

The camera constituted a possible source of constant error which ought to be considered before examining the results. In taking the pictures the film was exposed 16 times per second. Any phase of a lid- or eye-movement which occurred during the approximately 0.025-second interval between exposures could not be recorded. Whenever a movement maximum happened to occur within that interval the record would not show its full extent. The effect of this artificial shortening in some of the records is to make our mean results somewhat too small. But according to calculations based on the probable time relations of the lid and shutter movements, this reduction should be relatively slight and should have no effect whatever in 80 to 90 per cent of the cases.

Eye-Movement during the Blink

The data bearing on this question are restricted to the records of eye- and lid-movements during fourteen blinks by four of the six subjects. Usable data were not secured from subject No. 1 because of inadequacies in recording, nor from subject No. 2 because of failure to blink during the period of exposure of the film. Subjects Nos. 3, 4, 5, and 6 blinked respectively 4, 1, 2, and 7 times during the exposure of the film. Two of the blinks by No. 6 were double and are not plotted.

The data from the remaining twelve blinks are represented in Figure 1 as briefly explained in the legend below. The lid-separation graph at the top of the figure was constructed as follows: in case of

[5]The practice of periodically shifting the image was continued throughout the experiment to avoid tendency to stereotyped judgment.

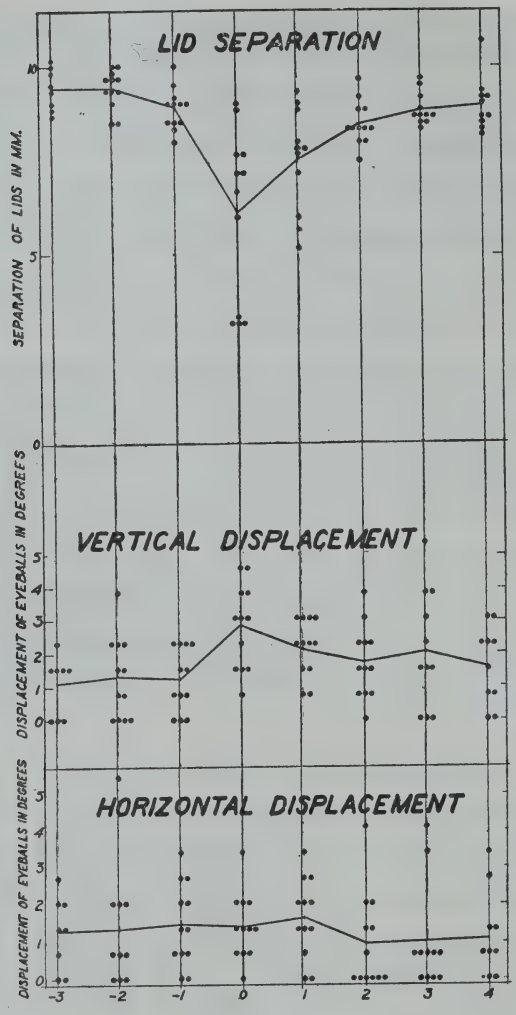

FIGURE 1

REFLEX LID-MOVEMENT

The three distributions represent respectively the individual measures of lid-separation in mm., vertical eyeball displacement in degrees, and horizontal eyeball displacement in degrees. These measures are indicated along the ordinates. In the vertical displacement distribution up signifies upward movement. In the horizontal displacement distribution up signifies toward the temple. Due to conditions of plotting explained in the text, the trend lines do not fall to zero. The distance between successive ordinates for all measures represents 1/16 second. Ordinate 0 indicates the moment of maximal lid-closure. Ordinates —1, —2, etc., indicate time intervals in 1/16 second preceding maximal lid-closure while ordinates 1, 2, etc., indicate similar time intervals following maximal closure. In each distribution, the plotted curve connects the successive means of the measures.

each blink, that motion frame containing the view of the eye at the moment of maximum closure was stilled and the amount of lid-separation measured and plotted. Then the immediately preceding frames were viewed and measured in sequence until a frame was reached beyond which no further lid-movement was perceptible. Similarly, the frames immediately following maximal lid-closure were measured and plotted. Thus the dots in the lid-separation distribution represent the measures of the twelve blinks throughout or nearly throughout their courses. The original plot was considerably wider than Figure 1 but the latter shows the essential facts. The broken line connects the means of the measures and shows the general trend. The vertical and horizontal ball displacements were measured from the same frames as the lid-movements and plotted on the same abscissae; therefore all three curves are in temporal coincidence.

The intervals of all the distributions or the ordinates of all the curves are equi-spaced at 1/16 second, but the recorded movements themselves not only vary considerably in duration but also may have been caught by the camera in different phases. Three consequences of these facts are essential to close interpretation of Figure 1 (or Figure 2). First, in none of the distributions can the plotted points represent precisely corresponding phases of the individual movements. Therefore, in the trend curves made by connecting the mean points the phase factor is mixed. If the individual movements were in phase the slopes would be more gradual. Second, the maximum number of points is plotted at the zero ordinate or moment of maximum lid-closure, but the number tends to decrease with departure from zero in either direction. For the data corresponding to any lid-movement are plotted only through the range of that movement; the data corresponding to shorter movements obviously can not be represented on the more extreme ordinates. Third, the ball-displacement curves connecting the means of the points can not be expected to drop to zero; such a curve would reach zero only in the remote event where displacement happened to be "zero" at the same ordinate for all cases.

There is a second factor which effectively prevents the displacement curves from ever reaching zero, regardless of length of baseline. This factor is the accidental error of measurement. The displacement measures required the setting of an arbitrary zero from which to estimate amount of eyeball movement. Zero displacement, for

purposes of plotting, was defined as the minimum measure recorded during the period of any given lid-movement. All displacements during a given lid-movement are, therefore, merely differences between the minimum or arbitrary zero measure and the other measures recorded during that lid-movement. Since the minimum measure is taken as zero, all measures exceeding it will be plotted above zero and the trend lines are above zero. Therefore, the best estimate of mean displacement from a curve is the difference between the low and high levels of the curve.

In the curve of vertical eyeball movements no gross change is apparent until the moment of maximal lid-closure. At that time the curve mounts from 1.3° to 2.9° with subsequent decline to 1.7°. The mean of the two differences is 1.4°, which is the most representative estimate of displacement from the curve. The curve of horizontal measures indicates no lateral roll with lid-closure although a slight medial movement is suggested as accompanying the final phase of the blink reaction.

While considerable variability in duration and extent of movement is evident from the the massed data of the distributions, the fact that the individual differences are somewhat greater than the differences within individuals is only evident from scrutiny of the original records.

Figure 1 was not plotted from the values in Table 1; the latter represents a fundamentally different treatment of the original measures as briefly explained in the note. This treatment made feasible the inclusion of fourteen blinks, two of which were definitely double or bimodal. These double blinks were omitted in Figure 1 because they would have confused the interpretation. Column one of the table indicates the subject making each given blink, column two the extent of the lid-movement in terms of mm. separation of the lids. Column three, M_p, presents the vertical ball-displacements which were estimated as follows: first, the mean position of the eyeball in the ten frames preceding the commencement of the lid-movement was calculated; then the difference between this mean or normal position and the position at the moment of minimum lid-separation was used as the estimate of displacement. This procedure was carried through for each of the fourteen blinks, though in a few of the longer blinks the mean had to be based on somewhat less than ten cases. The displacements in the fourth column, M_s, were esti-

TABLE 1

EYEBALL DISPLACEMENT, IN DEGREES, AS RELATED TO LID-SEPARATION, IN MM., DURING THE REFLEX LID-MOVEMENT*

Subject	Minimum lid-separation mm.	Vertical displacement M_p	Vertical displacement M_s	Horizontal displacement M_p	Horizontal displacement M_s
3	3.2	1.2	1.6	1.4	.9
3	3.2	3.7	2.2	2.	1.9
3	3.2	4.1	3.2	—.3	—.6
3	3.3	1.9	2.5	1.2	1.5
6	5.8	1.9	...	1.7	...
6	6.	.5	.5	1.5	.3
6	6.7	—.6	.2	1.1	—1.2
6	7.	2.6	...	1.3	...
6	7.2	2.6	1.8	—.5	—.5
4	7.2	1.9	3.6	—2.1	—1.8
6	7.7	.7	—.4	.5	—.3
6	8.	.8	...	2.3	...
5	8.8	1.2	.2	.6	—.5
5	9.	.8	1.6	8.7	11.1
Mean	6.16	1.66	1.54	1.38	1.00
Critical ratio		5.0	3.0	1.9	0.9
Percentage + cases		93	91	79	45

*The displacements under M_p are estimates from the mean of a series of frames, usually ten, just preceding the commencement of each lid-closure; under M_s are similar estimates from mean measurements succeeding the end of lid-opening. In case of the vertical measurements a minus sign (—) indicates downward displacement; the others are upward. In case of the horizontal measurements, a minus sign (—) indicates inward or nasalward displacement; the others are outward or temple-ward. The means of the last four columns show whether the movement was up or down, in or out, on the average, and how much in degrees. The critical ratio is the mean divided by its standard error *as adjusted* in accordance with the small number of cases.

mated in the same way with the exception that the mean of the ten measures *succeeding* the blink was used as the basis of estimate. The M_s values serve as a certain check on the M_p values, but the latter may be more representative because they are probably derived from a somewhat more stable baseline. The relatively slow return of the lids, and presumably the eyes, to normal after the moment of maximum closure makes uncertain the estimation of the point where normality is reached.

The same procedures were followed in estimating the horizontal displacements shown in the last two columns of the table. The several blanks in the M_s columns represent instances in which double

or quickly-following lid movements made impossible computation of a normal mean following closure.

Negative signs preceding vertical displacement values indicate that the estimated roll was downward below the normal position. Negative signs preceding horizontal displacement values indicate that the estimated roll was inward toward the median plane. The small number of negative signs indicates that in general the estimated displacement was upward and outward. The last row of the table shows the percentage of plus cases to be over 90 for upward displacement and about 60 for outward displacement. The critical ratios, adjusted for small *n*, show the upward roll to be statistically significant and the outward roll to be somewhat doubtful. According to the means, upward displacements of the order of 1.6° are to be expected, while any tendency to outward displacement should be definitely less. Evidently, these tabled results point in the same direction as the graphical results.

Eye-movement during the Wink

Each subject winked five times by instruction during the exposure period; and complete records of thirty winks became available for analysis. The data were recorded and studied in the same manner as the blink data. Figure 2 and Table 2 are constructed in precisely the same way as Figure 1 and Table 1. Except for difference in number of cases the blink and wink data seem to be strictly comparable. Before comparing them, however, the principal indications of the wink data may be summarized.

The generalized curve of lid-separation exhibits two peculiarities; a slight sag near the beginning of the wink and a continuous upward trend after maximal closure, bringing the curve well above the starting height. The sag is small enough to be accidental but the rising curve following maximal closure is not fortuitous. The individual curves for most subjects show a rise for several frames, at which point the height becomes comparable with starting height. Subject No. 4, however, on two occasions at the final phase of the wink opened her lids slowly to an unusual degree, a peculiarity noted by the experimenter at the time of occurrence. The terminal measures of these two winks suffice to raise the mean curve at the end considerably above the median.

The curve of vertical displacement rises 0.7° at the time of maximal

TABLE 2

EYEBALL DISPLACEMENT, IN DEGREES, AS RELATED TO LID-SEPARATION, IN MM., DURING THE VOLUNTARY LID-MOVEMENT*

Subject	Minimum lid-separation mm.	Vertical displacement M_p	Vertical displacement M_s	Horizontal displacement M_p	Horizontal displacement M_s
5	.7	—.8	—1.	2.3	1.7
5	2.2	1.	1.6	1.2	.5
3	2.7	4.8	4.	—3.7	—3.7
3	2.8	3.5	3.5	1.4	1.4
5	3.2	2.5	3.2	2.9	2.7
3	4.	2.8	2.5	1.4	.8
6	4.5	1.1	1.5	1.4	—.5
6	4.7	.5	.8	1.2	1.
1	4.7	2.6	3.8	2.4	.8
6	5.	.2	.5	.4	.5
6	5.	1.8	1.5	.4	.1
1	5.2	2.2	2.4	2.4	1.4
3	5.8	2.9	2.7	.8	1.
2	5.8	—2.5	2.1	—1.2	—1.
6	6.	3.2	3.2	.5	—.2
5	6.	—.9	.5	.6	.1
1	6.2	.3	1.2	1.7	1.5
4	7.3	0	...	—.3	...
3	8.	1.1	2.5	0	—.2
4	8.	—1.	...	.7	...
5	8.2	5.	5.2	3.7	2.9
2	8.3	1.4	1.2	—.6	—1.4
1	8.3	.7	1.4	1.5	.8
1	8.7	1.7	3.1	1.8	—.9
2	8.7	2.	2.1	.4	1.
2	9.	.3	.7	.1	—.3
4	9.3	—.2	—.4	—.5	—.5
4	9.4	.4	.5	—1.6	.1
2	9.7	.5	.4	—.1	.5
4	10.	.5	...	—.8	...
Mean	6.24	1.25	1.87	.68	.67
Critical ratio		4.1	6.7	2.5	2.6
Percentage + cases		80	93	70	67

*The displacements under M_p are estimates from the mean of a series of frames, usually ten, just preceding the commencement of each lid-closure; under M_s are similar estimates from mean measurements succeeding the end of lid-opening. In case of the vertical measurements a minus sign (—) indicates downward displacement; the others are upward. In case of the horizontal measurements, a minus sign (—) indicates inward or nasalward displacement; the others are outward or temple-ward. The means of the last four columns show whether the movement was up or down, in or out, on the average, and how much in degrees. The critical ratio is the mean divided by its standard error.

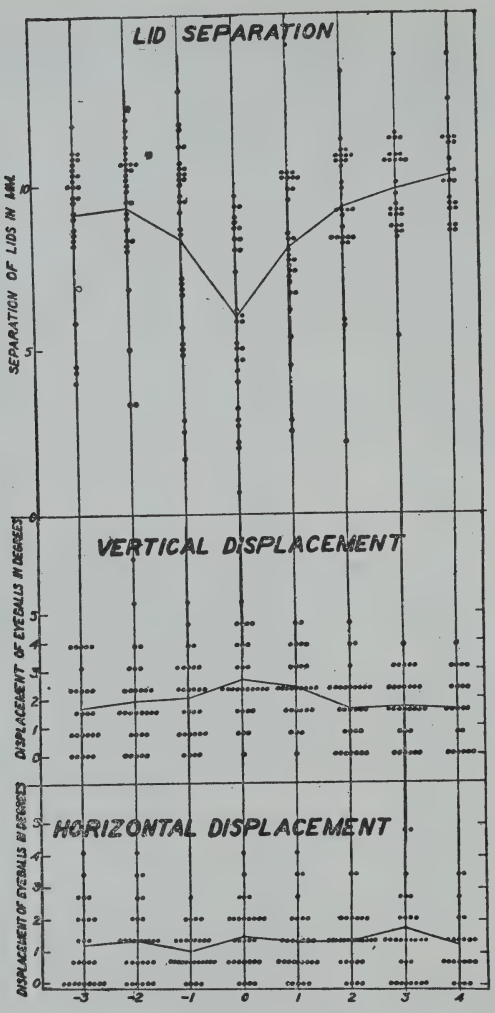

FIGURE 2

VOLUNTARY LID-MOVEMENT

The three distributions represent respectively the individual measures of lid-separation in mm., vertical eyeball displacement in degrees and horizontal eyeball displacement in degrees. These measures are indicated along the ordinates. In the vertical displacement distribution up signifies upward movement. In the horizontal displacement distribution up signifies toward the temple. Due to conditions of plotting explained in the text, the trend lines do not fall to zero. The distance between successive ordinates for all measures represents 1/16 second. Ordinate 0 indicates the moment of maximal lid-closure. Ordinates —1, —2, etc., indicate time intervals in 1/16 second preceding maximal lid-closure while ordinates 1, 2, etc., indicate similar time intervals following maximal closure. In each distribution, the plotted curve connects the successive means of the measures.

closure and then in two frames drops off 1.1° simultaneously with the opening of the lids. A mean displacement of the order of 1° is suggested. The horizontal curve shows a slight dip just before and another somewhat after the moment of maximal closure. In view of the variability and the procedure of plotting, these slight dips can not be considered significant.

Table 2 shows mean upward displacements of the order of 1.5° and mean outward displacements of less than 0.7°. According to the critical ratios the upward roll is statistically significant while the temple-ward movement is somewhat doubtful. In both instances the percentage of cases favors the upward-outward roll. Though these tabled results are more definite than the graphs, on the whole the methods are mutually confirmatory.

Comparative Summary of Results

Comparison of Figure 1 and Figure 2 shows considerably more varability in the winks than in the blinks. This relation is to be expected from the principle that voluntary activity is more variable than reflex. The general trend is much the same in both instances.

Tables and figures agree in indicating for both wink and blink a mean upward displacement of the eyeball of the general order of 1.5°. In mm. this amounts to approximately 0.33 at the corneal surface. The result is statistically significant and is supported by low Pearson correlation. The mean displacement appears to attain its maximum at or near the moment of minimum lid-separation.

For both wink and blink we have less certain evidence of a slight temple-ward displacement. This result is opposed to previous data by other investigators which suggested a nasalward roll at the time of lid-closure. Individual differences in muscle balance may account for this difference in results.

The small upward displacement is also in disagreement with previous results which indicated upward roll of the order of 10° to 15°. The fact that the subjects in the present experiment wore a lid-restraining device could scarcely be expected to account for this gross difference in results. Still, there remains the possibility that with complete free closure of both lids a definitely greater roll of the ball might occur. Experiments in progress may settle that point and throw more light on the seemingly synergic relation between eye- and lid-movement.

Investigations by Dodge and by McAllister of the irregularities of fixation show variations of less than 1° during attentive regard of a small cross or fixation point (1, 2). That in conjunction with our results indicates the important technical possibility of strictly continuous stimulation of the central portions of even moderately sized retinal areas.

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The Clinic of Child Development
Yale University
New Haven, Connecticut

LES MOUVEMENTS DE L'OEIL CORRELÉS AVEC L'INNERVATION DE L'ORBICULARIS OCULI

(Résumé)

On a étudié chez six sujets au moyen de la technique cinématographique les mouvements de l'oeil humain coincidents avec le clignotement réflexe et les clins d'oeil volontaires. On a obtenu de l'évidence statistiquement signifiante d'un déplacement moyen de la pupille dirigé en haut, au moment du mouvement de la paupière, de l'ordre de 1,5%. Il s'est montré de moins certaines indications d'un petit déplacement vers la tempe. Il paraît que les deux résultats ne s'accordent pas avec les données antérieures qui indiquent qu'un beaucoup plus grand roulement en haut en combinaison avec un petit mouvement vers la narine est corrélé avec des mouvements libres de la paupière.

Newhall et Halverson

AUGENBEWEGUNGEN MIT INNERVATION DES ORBICULARIS OCULI KORRELIERT

(Referat)

Bewegungen des menschlichen Auges, die Reflexblinzeln und willkürliches Blinzeln begleiten, wurden bei sechs Vpn. vermittelst einer kinematographischen Methode untersucht. Statistisch bedeutsame Evidenz einer Durchschnittsverrückung des Augapfels nach oben zur Zeit der Lidbewegung der Ordnung von 1,5° wurde bekommen. Es ergaben sich weniger sichere Andeutungen einer geringen Verrückung nach den Schläfen hin. Die beiden Resultate sind in offenbarem Widerspruch mit früheren Ergebnissen, die zeigen, dass ein viel grösseres Drehen nach oben in Verbindung mit einer geringen Bewegung nach der Nase hin mit unbeschränkten Lidbewegungen korreliert ist.

Newhall und Halverson

A METHOD FOR INVESTIGATING THE SENSITIVITY OF THE SKIN TO MECHANICAL VIBRATION*

From the Psychological Laboratory of the University of Virgina

FRANK A. GELDARD AND B. VON HALLER GILMER

Within the past dozen years there have been described numerous devices designed to test the sensitivity of the skin to mechanical vibration. Following the trend of recent technological advances in electrical knowledge most of the more nearly adequate of such devices have employed oscillator-driven vibrators (3, 4, 6, 8) though some have continued the use of tuning forks (5). Within the clinical field the latter have been used almost exclusively (7) in spite of their essential inadequacy, which has been long recognized. Notwithstanding the varied approaches that have been made to the general problem there still exists a paucity of concrete data of an exact sort on skin sensibility to vibration. This situation arises not so much from a lack of fundamentally sound design in instrumentation, though to this is attributable much of the inconsistency in results, as to the very obvious failure of most investigators to recognize the complexity of conditions that obtain when an object in vibration contacts the skin.

While the functional possibilities of the elaborate cutaneous receptor system are as yet little understood, this fact constitutes no adequate excuse for neglect to control rigorously the conditions of stimulation. It is apparently true that the skin has been regarded as a kind of neutral coupling device conveniently lying between the nervous system and the external physical stimuli for "vibration-feelings." This situation stands in surprising contrast to that found in any other field of sense psychophysiology. At least, similar failures in control may there be laid to inaccessibility of receptors; this condition is not realized in work on the skin. It is obvious that we shall be no nearer solution of even the most general of problems to be encountered in this field, for example the resolution of the difference of opinion concerning the very nature of vibratory sensibility, viz., whether it

*Accepted for publication by Carl Murchison of the Editorial Board.

is mediated by the "pressure sense" or whether a special "vibration sense" must be invoked, until a technique incorporating adequate refinements of control is utilized. The following account represents a step in the direction of the provision of such a technique. It may be stated in advance that the method to be described is not merely a projected one but one which has decisively proved its adequacy in the solution of a number of basic problems of measurement throughout the course of several months of practical use.

The features desirable in any apparatus for the investigation of skin sensitivity to mechanical vibration include the following:

1. A contactor which (*a*) is capable of application not only to the finger tips, the area most commonly selected, but also to the less accessible areas of the skin or (*b*) with which the more easily manipulated areas can be brought into contact. It is desirable that contact mediums of different sizes be provided for the investigation of skin areas of correspondingly different size. The contacting mediums should be of such a nature as to insure uniform application to different topographical areas of any given general region. For example, in stimulating two different areas at the finger tip the contactor must in both cases rest on the skin in exactly the same manner.

2. A driving mechanism for the contactor capable of wide frequency variation and of consistently producing a good wave form, presumably one of simple harmonic motion (sinusoidal wave). The amplitude of the swing of the contactor must be capable of variation in small increments over a wide range. Avoidance of wave distortion is necessary at all amplitudes; this becomes particularly important at the higher frequencies and higher amplitudes. Both the frequency and amplitude must remain constant for any given setting of the apparatus. There must be adequate means of measuring any damping effect the skin may have upon the operation of the contactor.

3. A recording system which makes possible a determination of the extent and nature of the excursion of the contactor *at the point of contact with the skin.* It is desirable that the recording system make possible the obtaining of permanent records of stimulating conditions in critical experiments and of transient phenomena that could not otherwise be analyzed.

4. A means of minutely observing any given skin area in order to make possible the noting of mechanical skin changes that occur

during stimulation. Local topographical areas of the skin must be mapped accurately to permit exact repetition of a particular experiment. The amount of skin depression due to the contact point must be kept constant for any given experiment.

5. A calibration system of high accuracy and reproducibility.

Apparatus has been constructed meeting all of the above demands. The essential features are shown in simplified and schematic form in Figure 1.

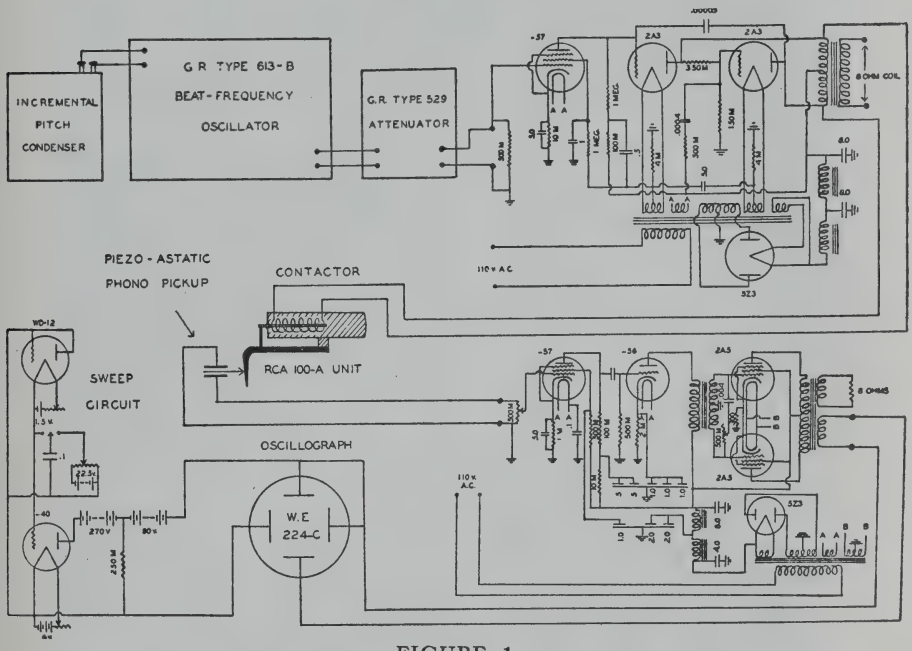

FIGURE 1

Schematic Diagram of the Apparatus

The Contactor

The contactor consists of an RCA, Type 100A magnetic speaker unit modified in such a way as to permit a needle to vibrate as an extension of the armature. This speaker unit possesses a good frequency response over a wide range and has the advantage of being capable of handling fairly large powers. It probably gives as faithful a response as characterizes any speaker of the magnetic type. Un-

questionably the best speaker unit available is the Western Electric, Type 555W receiver (11) which gives the greatest fidelity of reproduction of any speaker yet devised. It is practically free from harmonics even when carrying great loads. Because of its superior characteristics an attempt was made to use this unit as a vibrator. However, difficulty was encountered in making a suitable attachment of a needle to the delicate aluminum diaphragm. Under these conditions the unit operated over a wide range with relatively few harmonics, but there were introduced disturbing longitudinal and sidewise vibrations. These factors made it impossible to insure constant stimulating conditions for wide variations in frequency and amplitude. In addition this unit proved too bulky to be used conveniently.

Detachable aluminum points one to six millimeters in diameter are used as contacting mediums in stimulating skin areas of different size. When the point of the needle itself is employed as a stimulator a dummy rider is placed on the needle to insure constant loading of the contactor. The diameter of the needle point is approximately .5 mm. The hand, arm, or finger tip is brought into contact with the contactor by means of a rack and pinion device which regulates a movable platform. When the surface of the arm is to be stimulated the elevating mechanism is fitted with a plaster cast. In cases where the vibrating point itself has to be moved into contact with the skin, whether by reason of difficulty of access to the skin area one desires to stimulate or for purposes of insuring comfort to the observer, it is a relatively simple matter to mount the contactor and its attachments on an adjustable platform, moving it as a unit. Some caution has to be observed in the matter of providing effective padding to prevent vibrations from reaching the skin other than through the contactor point; liberal quantities of sponge rubber and thick felt were found adequate to meet this need. The weak tone produced by the speaker is satisfactorily masked out by the noise of a conveniently situated electric fan.

An important feature of this vibrating device is that it is capable of variation in a number of ways and that it performs in a constant fashion for a given setting of the controls. Inadequacies in fundamental design may be corrected for by temporary loading of the needle. This procedure is followed only at the higher frequencies in order to bring the armature of the speaker into better relation

with the magnets. As will be seen minor variations of this sort are permissable, since the recording system gives a true picture of the stimulating conditions at all times.

The Driving Mechanism

The driving mechanism consists of a General Radio Beat-Frequency oscillator, Type 613-B, equipped with an incremental pitch condenser.[1] This oscillator generates frequencies from 5 d.v. to 12,500 d.v. and is relatively free from harmonics. This instrument is calibrated against a 100-cycle reed each time it is used, thus insuring very accurate frequency settings. The output of the oscillator is further amplified with a two-stage Lafayette 2A3 push-pull amplifier. The chief advantage in using this amplifier is that it gives a large power factor and is distortionless. The circuit consists of a —57 vacuum tube direct-coupled to two 2A3 vacuum tubes in push-pull. Between the oscillator and the amplifier is interposed an "L"-type attenuator calibrated in decibels. This, together with the output control of the oscillator, a graduated potentiometer, makes possible the obtaining of any of a wide range of amplitudes. The use of the amplifier incorporating the new 2A3 tubes makes possible high amplitudes devoid of distortion of the relatively pure sine waves produced by the oscillator.

Recording and Measuring Device

It is one thing to have knowledge of the characteristics of the current activating the contactor but quite another to be able to observe the frequency, amplitude, and wave form with which the contacting device is stimulating the skin. Previous investigators without exception have been content to do the former. The apparatus makes possible very exact determination of frequency, amplitude, damping, and wave form *at the contactor.*

The first recording system installed consisted of a modified arrangement of the phonodeik. This method proved unsatisfactory because of the inertia set up within the system itself. Recourse was next made to the more direct principle of the optical lever. This consisted of a small triangular stellite mirror suspended on a phospher-

[1]The authors wish to make known their grateful appreciation of a grant from the National Research Council which made possible the obtaining of the oscillator, condenser, and attenuator incorporated in the apparatus.

bronze strip held in position by an adjustable stirrup after the manner described by Wood (12) and used by Kennelly for testing telephone diaphragms. One apex of the mirror rode on a highly polished steel platform mounted to the needle in such a way as to vibrate with the needle. A beam of light was focused from the vibrating mirror onto a revolving mirror and thence to a screen. The failure of this mirror arrangement to follow the vibrating needle at high frequencies (those above 1000 cycles), because of its own critical frequency period, brought about the rejection of the optical lever method and the use of an electrical recording system.

A high fidelity piezo-astatic phono pickup was attached to the vibrating contactor in such a manner as to cause the whole mechanism to operate as a unit. The mechanical vibrations are thus imparted against the rochelle salt crystals of the pickup. These crystals in turn generate a small electric current which is then led through a 250,000 ohm potentiometer to the grid and filament of a —57 vacuum tube in the first stage of a three-stage amplifier. The —57 is capacity-coupled to a —56 tube which is in turn transformer-coupled to two 2A5 tubes in push-pull. As the rochelle salt crystals vary in distortion with extreme temperature changes (9, 10) caution must be observed in maintaining the surroundings at a relatively constant temperature. The output of the amplifier is led directly to two of the electrodes of a Western Electric, Type 224-C cathode ray oscillograph tube. The extent of deflection of the ray by the oscillatory changes imparted to the plates is a measure of the amplitude of the contactor vibrations. The other pair of plates connect with a sweep circuit, in the main described by Davis (2), but altered here to provide repetitive sweep. Thus frequency and wave form are directly read.

The entire recording system thus provides for measurement of all of the characteristics of the vibratory motion of the contactor *during stimulation* and in addition makes possible a determination of the extent to which the contactor is damped through contact with the skin. This matter is one of considerable importance and has been entirely neglected in most previous work. The extent of damping is necessarily a variable factor calling for rigorous control if trustworthy threshold measurements are to result. Such control is impossible in an apparatus not employing an independent system of recording the excursions of the contactor.

The use of the cathode ray oscillograph further permits the obtaining of permanent records. The path of the fluorescing ray may be photographed; thus transient phenomena may be objectively recorded.

Calibration

While the recording system is relatively perfect in that it makes possible measurement of various characteristics of the vibration, the method is not, of course, a directly metrical one. The characteristics of the pickup, the amplifier, and the oscillograph are such as to necessitate calibration. This has been accomplished by a method not unlike that employed by Knudsen (6), but one which represents an elaboration of his device to permit greater accuracy.

The calibration apparatus consists of a lever 200 cm. in length mounted on a knife-edge fulcrum. This lever is so mounted as to give a ratio of 190 cm. to 10 cm. between the two arms. A graduated micrometer screw is mounted to the end of the longer arm of the lever. A rounded steel point attached to the shorter arm of the lever serves as one contact for a local electric circuit. The rounded screw head of the detachable aluminum point on the contactor needle serves as the second contact. With these a 1.5 volt dry cell and a pair of high sensitivity Baldwin headphones are connected. The current used here must be small enough to avoid any arcing across the two contacts. The exact point of meeting of the two contacts is accurately determined by listening for the first click in the headphones. A "zero" setting is thus obtained by adjusting the micrometer screw just to the point where contact is made with the resting contactor needle. The two contacts are then separated and the contactor set in motion. The contacts are again brought into connection with one another and the point of contact read directly from the micrometer screw. Amplitude measurements are made from the top and bottom of the contactor swing. This procedure serves to avoid any spurious readings which might otherwise result from "off-centering" of the contactor while vibrating. Thus amplitudes are directly measured to cm. $\times 10^{-6}$. The amplitudes are then calibrated against the sweep (in mm.) of the oscillograph record. Calibrations are made for each frequency with regard to the output control settings on the oscillator and the attenuator and for the potentiometer settings on the pickup amplifier. Calibration curves

for each frequency and each detachable contact point are thus obtained. A careful check proved that there are no measurable sidewise or longitudinal motions of the contactor.

An alternate method of calibration involves a micrometer scale in a high magnification ocular of a microscope. This method necessitates the addition of some small, rigid attachment of no appreciable weight to the contactor point. This will vibrate with the contactor and produce a clear outline which can be measured in the microscope. For this purpose is suggested a small, hollow glass tube drawn out to approximately the size of a horsehair. Whereas this method is not as sensitive as the one described above we find that extrapolation of the readings in the direction of the smaller amplitudes shows remarkable agreement with those obtained by the lever method.

A still more sensitive method, which was not attempted but whose possibilities were investigated, involves the use of interference fringes, and yields readings in terms of the wave-length of light. This method would present some difficulties of manipulation but is not entirely impracticable. If the occasion for need of greater accuracy arises it presents real possibilities.

Topographical Skin Areas

The examination of local features of the skin is made through the use of a Spencer binocular dissecting microscope. A magnification factor of 86.6 x was found most convenient. The microscopic field is illuminated by a 1000-watt lamp, water filtered, and placed some distance from the contactor to avoid heat disturbances. Microscopical study of the skin surface is a most important matter in that sensitivity is found to differ markedly from one local topographical area to another. Without the use of a microscope one cannot accurately restimulate a particular skin area from time to time. In order to assure further accuracy in mapping given areas of the skin there has been employed the method described by Dallenbach (1). In order to make permanent maps of different skin areas the part in question is brought into contact with an ink pad so that a thin film of ink is left on the skin. The area is then pressed upon appropriate paper.

The binocular microscope has the advantage of making possible a measurement of skin depression. One ocular may be focused on the skin when "normal." The other ocular is focused on the skin when at its limit of depression. Thus one can by so focusing the

two oculars to different levels obtain a means of keeping the skin depression constant. This is important since different types of contacting mediums cause different kinds and amounts of skin depression.

It is obvious that there exists a number of possibilities in the application of the method to the study of the vibratory reactions of the skin. In actual use there is required as an adjunct some formal procedure such as is supplied by any of the conventional psychophysical methods. We have found a modification of the Method of Limits to be convenient but there is nothing inherent in the apparatus which precludes the use of the others.

Already the method has been successfully employed to determine the basic amplitude-frequency function for the entire range of vibratory sensitivity with results that indicate gross inaccuracies in former measurements. An account of this experiment, together with certain extensions of it, will provide the content of an early communication.

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University of Virginia
University, Virginia

UNE MÉTHODE POUR L'INVESTIGATION DE LA SENSITIVITÉ DE LA PEAU À LA VIBRATION MÉCANIQUE

(Résumé)

On donne un résumé des caractéristiques désirables dans n'importe quelle méthode pour tester la sensitivité de la peau à la vibration. Celles-ci comprennent un appareil qui emploie un moyen de contact capable d'une application facile à une variété des aires de la peau, un méthode de faire marcher le moyen de contact à diverses fréquences et amplitudes, un système d'enregistrement qui indique l'étendue et la nature de la stimulation de la peau, des facilités pour l'observation exacte des changements mécaniques de la peau, et un système de calibration d'une grande précision et reproductibilité. On décrit un appareil et un procédé qui satisfont toutes ces exigences. L'appareil se compose essentiellement de la partie mécanique d'un haut-parleur électromagnétique modifiée pour permettre la stimulation de la peau au moyen d'une extension de l'armature, la force motrice étant le courant amplifié d'un oscillateur "beat frequency." Pour rendre certain qu'on obtiendra des données des vraies conditions stimulantes à la surface de la peau, on a incorporé un système d'enregistrement qui utilise un cristal piezoélectrique, un amplificateur, et un oscillographe. Ce système mesure non seulement la fréquence, l'amplitude, et la forme de l'onde mais aussi indique le degré d'amortissement par la peau elle-même. Une technique de calibration suffisante rend certaine la précision de la mesure. On promet très tôt un rapport des études faites selon cette mèthode.

GELDARD ET GILMER

EINE METHODE ZUR UNTERSUCHUNG DER EMPFINDLICHKEIT DER HAUT BEI MECHANISCHER VIBRATION

(Referat)

Die wünschenswerten Eigenschaften irgendeiner Methode zur Prüfung der Hautempfindlichkeit gegen Vibration werden entworfen. Diese erfordern einen Apparat, der eine Kontakteinrichtung zur leichten Berührung mit allerlei Hautflächen ermöglicht, der zum Anschlagen des Kontaktpunkts zu verschiedenen Häufigkeiten und Schwingungsweiten gestattet, ein Registriersystem, das den Umfang und die Natur der Hautreizung angibt, Möglichkeiten zur genauen Beobachtung der mechanischen Hautveränderungen, und schliesslich ein Kalibrationssystem einer hohen Genauigkeit und Wiederhervorbringung. Ein Apparat und ein Verfahren werden beschrieben, die all diese Forderungen befriedigen. Der Apparat besteht wesentlich aus einem elektromagnetischen Sprecher, der modifiziert ist um die Hautreizung durch eine Verlängerung der Armatur zu gestatten. Die Maschine wird durch den amplifizierten Strom eines Schlag-Häufigkeit-Schwingungsapparat (beat-frequency oscillator) betrieben. Um zu versichern, dass die Daten über die wirklichen Reizzustände auf der Hautflächen erhalten werden sollten, wurde eine Registriersystem eingerichtet, das einen piezoelektrischen Kristall (piezoelectric crystal), einen Amplifikationsapparat, und eine Schwingungsmaschine (oscillograph) gebrauchte. Dieses System misst nicht nur die Häufigkeit, Schwingungsweite, und Wellenform, sondern zeigt den Grad des Dämpfens durch die Haut selbst an. Eine genügende Kalibrationsmethode versichert die Präzision der Messung. Ein künftiger Bericht über die durch diese Methode ausgeführten Untersuchungen wird versprochen.

GELDARD UND GILMER

DOUBLE MONOCULAR DIPLOPIA*

From the Psychological Laboratory of the University of Kansas

D. M. PURDY

I

In normal visual function, each point of either retina has its "corresponding" point on the other retina, which is situated in an approximately similar position on the retinal surface. Stimulation of either one of a pair of corresponding points, or of both at once, gives rise to a visual impression which is subjectively localized in the same direction in space. We have single binocular vision when the two retinae receive similar images which fall on corresponding points, or on points which depart only slightly from exact correspondence. There are strong reasons for believing that the correspondence of the two retinae has a congenital basis (8, p. 248 ff.).

When, as in strabismus (squint), the two eyes are unable to adjust their convergence so as to bring the two retinal images of an object upon corresponding areas, one might expect that continual double vision would be the consequence. This is sometimes the case. In many squinters, however, this trouble is avoided by an inhibition of perception in the squinting eye, so that vision is essentially monocular.

There is still a third class of squinters who escape diplopia in another way: by the development of an unusual or "anomalous" mode of cooperation between the two eyes (4, 7, 17). The squinting eye—or, to speak more accurately, its cerebral apparatus—develops a new system of spatial perception. In this new system, a certain point on the periphery of the retina has the same subjective "space-value" as the center of the fovea of the opposite eye; and the space-values of all other retinal points are changed accordingly. By virtue of this reorganization, any visual object is local-

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ized in identical fashion by both of the eyes, and thus binocular single vision is achieved.[1]

Such an acquired system of binocular cooperation is sometimes called an "anomalous correspondence" of the retinae. It has frequently been pointed out, however, that an anomalous system is not strictly comparable to the normal or congenital correspondence. There is not usually a precise point-for-point correspondence, as in normal vision, but rather a somewhat variable and unstable relationship. Most (according to some authorities, all) of the squinters having an anomaly of space-values fail to display the binocular fusion-movements and the binocular stereoscopic vision which accompany the normal mode of interworking of the two eyes. Hence many writers, following Tschermak (16), speak of an "anomalous community of visual directions" (*anomale Sehrichtungsgemeinschaft*) rather than of an anomalous correspondence.

Now, there are certain rare individuals who display this anomalous mode of localization *in addition to* the normal mode, and at the same time. In these curious cases, a single object stimulating the retina of one eye is simultaneously localized in two different ways. In other words, there is *monocular diplopia.* The first case of this sort seems to have been observed by Javal (9) in 1865. The significance of the phenomenon was pointed out some years later by Hering and Bielschowsky.

As a rule, monocular diplopia has been found in patients whose squint had been corrected by operation. In this situation one might well expect a "reawakening" of the normal perceptual system. Evidently, the anomalous system may linger on even after the normal system has been restored.

Bielschowsky (2), who worked under the direction of Hering, reported the first thorough study of monocular diplopia in 1898. His subject was a squinter who had lost one eye by operation. In the eye which remained, there had formerly been a local inhibition of perception in the foveal region; and with the removal of this

[1]The magnitude of the shift in space-values, expressed in angular measure, is called the "angle of anomaly." If binocular single vision is to be realized, the angle of anomaly must of course be equal to the angle of squint. This is not always the case. When the two angles disagree, it is generally assumed that the angle of squint has altered since the time when the anomalous system was developed, and that the system has failed to adapt itself to the changed situation.

inhibition as a result of the enucleation of the opposite eye, monocular diplopia appeared. Bielschowsky was able to show that the phenomenon could not have any physical basis in peculiarities of the dioptric mechanism of the eye. A monocular diplopia sometimes appears in non-squinters, due to some ocular abnormality such as a double pupil.[2] The monocular diplopia of squinters, however, displays many features which differentiate it clearly from any diplopia of physical origin. Bielschowsky performed one experiment which is particularly interesting. If two objects of different color were presented, side by side, each gave rise to two visual impressions, one "normally" and the other "anomalously" localized. By appropriate placement of the two objects, the "normal" impression of one object could be brought into apparent coincidence with the "anomalous" impression of the other object. Under these conditions the subject experienced phenomena of rivalry, analogous to those of normal binocular vision, as well as phenomena of color-mixture.

When the subject was re-examined 16 years later, the diplopia was still in evidence (4, p. 1106).

Other cases of monocular diplopia have been reported by Javal (10) and Bielschowsky (3, 4), and by Ohm (13), but the phenomenon seems to be quite uncommon. Thus, Worth (19) writes that in his examination of more than 2000 squinters he has noted only four cases of monocular diplopia.

In all cases of monocular diplopia of which I have found descriptions in the literature, the phenomenon was displayed by one eye only.[3] As a rule, one eye assumes the rôle of a leading or dominant eye, and is habitually employed for foveal fixation. Anomalous localization develops in the opposite eye, in such a way as to bring its space-values into harmony with those of the dominant

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[2]Monocular diplopia has also been found in cases of cerebral lesion (1;14, p. 358f.;18) and as a transitory symptom in encephalitis epidemica (5). The origin of the phenomenon in such cases is an interesting but very enigmatic question. According to the recent experiments of Schumann (15), phenomena of monocular diplopia can be aroused in normal subjects under certain special conditions.

[3]The physiologist A. Tschermak, who is himself a squinter, reports that he experiences monocular diplopia in either eye under certain special conditions. If he closes either eye, and endeavors with this closed eye to "fixate" an object in the field of the open eye, the objects in that field appear double (16).

eye. If monocular diplopia is displayed, it appears only in the subordinate eye.

II

The case which is now to be described seems to be unique of its kind, in that the phenomenon of monocular diplopia is displayed by each of the eyes.

The subject, L, is a university student, who was 18 years old at the time I had an opportunity to examine him (1933). His peculiarity came to my attention by chance during an elementary laboratory class in psychology. In the brief investigation which followed, L showed himself to be an unusually careful and acute observer. It may be remarked that he is a student of exceptionally high intelligence and scholastic standing, with special interests in the physical sciences and a very considerable amount of scientific training.

During the first few years of his life, L had a very pronounced convergent strabismus.[4] When he was five years old, an operative correction was performed. At the time of this operation, the squint amounted to about 25°. After the operation, there was a residual convergence of about 4°.

Nine years later a second operation was performed, not to improve the vision, which had been satisfactory, but for cosmetic purposes. At the present time there is no noticeable squint, but appropriate tests indicate that a slight amount of convergence still remains. According to the data which will be given below, this amounts to about 2°. There is in addition a vertical divergence of the optic axes, also about 2° in magnitude.

Vision in both eyes is good, although there is a slight refractive error, the left eye being myopic and the right eye hypermetropic. This anisometropia is probably important in connection with the interpretation of the perceptual phenomena. At the present time the subject wears a lens of —1.5 diopters spherical power on the left eye, and a lens of +0.25 diopter spherical and 0.25 diopter

[4]My thanks are due to Drs. Walter B. Lancaster, of Boston, Mass., and Lyle S. Powell, of Lawrence, Kansas, the physicians who have been concerned with the case, for their interest in this study and their courtesy in supplying clinical data regarding L. I am also indebted to L for his willing cooperation.

cylindrical power on the right eye. With this correction the visual acuity is 20/15 in each eye.

In binocular vision, the subject experiences double images. These images are separated not only horizontally but vertically, the image on the left appearing higher than the one on the right. This appearance is of course exactly what one would expect in the case of a subject with a horizontal and vertical squint who possesses the *normal correspondence* of the two retinae.

The horizontal separation of the images is about 2°, and their vertical separation is about equally large. These separations were roughly determined by having L fixate a mark, then moving a pencil about until L reported that one image of the pencil apparently coincided with the contralateral image of the mark; the distance separating the pencil from the fixation-object was then measured.

Under ordinary conditions, according to L, he "neglects" one of these double images, without actually inhibiting it from the field of consciousness. Thus, if he happens to close one eye, he notices the disappearance of its image. It appears, however, that in any situation where the image of the non-fixating eye might interfere with efficient vision, this image is, as a rule, completely inhibited. When he is reading, for example, L's vision is strictly monocular. In detail-vision of near objects, L ordinarily sees only with the left eye, and in detail-vision of distant objects, he sees only with the right eye. This inhibition of one eye's impressions is under his voluntary control; he can always relax it and see simultaneously with both eyes—although he then, of course, often has great difficulty in making out fine details.

We turn now to the phenomena of monocular vision. Under appropriate conditions, L also sees double images with either eye alone.[5] The relative placement of these images is exactly the same as in binocular vision, the left image being higher than the right image. There is, however, a significant difference in the appearance of one of the images, which we shall mention shortly.

This monocular diplopia is especially striking to the subject when he is placed in a completely dark room and a small bright object is presented to the eye. It is, however, quite easily observable in

[5]Both the binocular and the monocular diplopia are unaffected by removal of the subject's correcting glasses.

the case of any object which contrasts fairly sharply with its background.

Thus, the separation of the images could easily be measured by means of the method described above in the case of the binocular images. With either eye the separation of the images was found to be the same as in binocular vision, namely, 2° horizontally and 2° vertically.

To either eye, one member of the pair of images is distinct, stable, and vivid; the other is less distinct and is easily inhibited. L spontaneously describes the first image as the "real" image, the second as the "unreal" image. In the case of a colored object, this "unreal" or unstable image appears in a less saturated color than the stable image. The saturation is so weak that the color generally has no identifiable hue; the color is just recognizable as "chromatic" rather than gray.

According to L, the unstable image is not "noticed" in monocular vision under ordinary circumstances, but it can almost always be observed as soon as he directs his attention to it. Even with both of the eyes open, he can obtain the phenomenon of monocular diplopia by voluntarily suppressing the vision of one eye. In place of the two "real" images of binocular diplopia, he then sees one "real" and one "unreal" image.

For the left eye, the unstable image is the one on the *right;* for the right eye, the unstable image is the one on the *left.* As we shall see, in each case the unstable image is an *anomalously localized image,* while the stable image is localized in accordance with the principles of congenital correspondence. L's reports regarding the hazy and evanescent character of the former image agree with those which have been made in other cases of anomalous localization.

For either eye, the unstable image has exactly the same location in visual space as the stable image of the opposite eye. Thus, whenever L observes an object first with one eye closed, and then with both eyes open, the unstable monocular image gives way to a stable image, furnished by the opposite eye, and appearing in exactly the same place.

The relation between the phenomena of monocular and binocular vision can be most clearly summarized by describing the results of a test made with the "congruence-apparatus" devised by Tschermak (17, p. 964) for the study of strabismus.

By means of this apparatus, one presents simultaneously a small illuminated cross, which the subject views with both eyes, an illuminated slit directly above the cross, which is seen by the left eye only, and a second slit directly below the cross, which is seen by the right eye only (Figure 1, *a*). The normal orthophoric subject of course sees the three objects as single and as lying in the same vertical line.

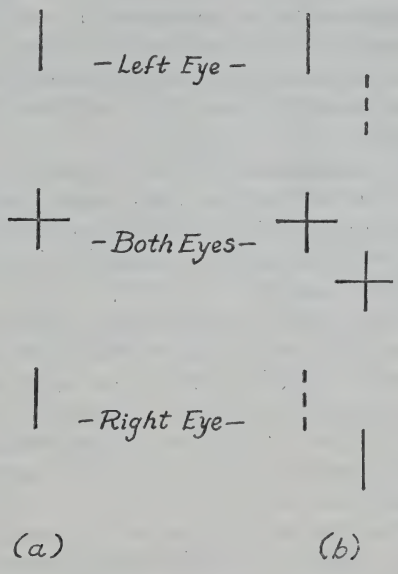

FIGURE 1

The appearance of the field to the subject L is represented in Figure 1, *b*. Images of the stable type are indicated by continuous contour-lines, images of the unstable type by dashed lines.

Each of the slits is seen monocularly double, while the cross is seen binocularly double. In each of the three cases, the separation and orientation of the double images is exactly the same; thus the subject sees two rows of images, one to the right of and below the other, both rows being exactly vertical and exactly similar to one another.

In monocular vision, one of the images is unstable; in binocular vision, both images are stable. Except for this difference, the ap-

pearance of the images is the same in binocular vision as in monocular vision with either eye. It will at once be clear from the diagram why this is so. In binocular vision, there is an exact superposition of the two pairs of monocular images. If one imagines the upper and lower pairs of slit-images to be combined, it is obvious that they would yield a single stable pair, such as is given by the central cross.

The interpretation of these phenomena is not difficult. As we have already remarked, the binocular appearances are entirely normal; the two images are localized in accordance with the principles of congenital correspondence, and their separation is determined by the angle of squint.

The fact that, in monocular vision, two images are seen instead of one, means that each of the eyes possess, in addition to the congenital system, an acquired or anomalous system of localization. Thus each eye sees an anomalous (unstable) image in addition to the normal (stable) image.

As Figure 1 illustrates, the anomalous system of each eye is of such a type as to yield precise agreement of space-values with the normal image of the opposite eye. If one eye possessed *only* the normal image, and the other eye possessed *only* the anomolous image, it is obvious that the fusion of these two images would yield binocular single vision; and L would simply be a typical case of a squinter with anomaly of visual directions.

In reality, L has two such systems of "anomalous community of visual directions," either of which, taken alone, fulfills the conditions for binocular single vision—and with both these systems coexisting as they do, he still experiences binocular diplopia.

The acquisition of an anomalous system of localization is often regarded as a functional adaptation which occurs in the interests of binocular single vision. But L's binocular perception is substantially the same as it would be if no anomalous system had been acquired. This suggests that, whatever functional value may accrue to an anomalous system in certain cases, this value has nothing to do with the origin of the system.

III

The question of the process by which anomalous localization is developed is one of great theoretical interest, which is still unsettled.

Several hypotheses have been proposed, none of them entirely convincing; and I shall not here attempt to add to these speculations.

In the particular case of L, it is entirely probable that his peculiar perceptual system has existed, in substantially its present form, since a very early period of life. L has had double binocular vision as long as he can remember; and he is very positive that this was the case at the time just previous to the first operation on his eyes. It is altogether likely that monocular diplopia was also present during this early period; and that the two operations have played no part in the causation of the phenomenon, but have simply entailed a modification of it, which I shall mention below.

In L's case, each of the two eyes is, and probably always has been, employed about equally for foveal fixation. Thus there was no pronounced dominance of one eye, as in the other cases of monocular diplopia. The right eye, being slightly farsighted, is habitually used for fixation of distant objects, and the nearsighted left eye for fixation of near objects. (The refractive defects of both eyes are now thoroughly corrected, but there is probably a persistence of tendencies established early in life.) Thus, this particular type of inequality between the two eyes has probably prevented the development of any marked dominance, and hence has made possible the acquisition of two anomalous systems.

Although L's anomalous systems were probably developed when the angle of squint was much larger than it is now, they have adapted themselves to the changed conditions. Each time the angle of squint was reduced by operation, a perceptual readjustment occurred. When the angle was diminished from 25° to 4° by the first operation, L would have experienced quadruple rather than double vision, if the anomalous systems had not changed to suit the new conditions. Actually, according to his report, he experienced double vision, just as at present.

The second operation reduced the convergence to a still smaller value. L declares that he has a very distinct memory of a quadruple vision which persisted for about a month after the operation. (During this period, he recalls, he had to give up such games as tennis, because he could not coordinate his movements to his fourfold perceptions.) But, as Figure 1 illustrates, the anomalous systems are now completely adapted to the present angle of squint. The separation of the images in monocular diplopia is equal to the

separation of the images in binocular diplopia—in other words, is equal to the angle of squint.

IV

In addition to his monocular diplopia, our subject L displays another striking peculiarity: an absence of the normal phenomena of *binocular fusion.*

Fusion was tested by the use of the stereoscope. In L's case, where there are two perceptual images for each eye, one has a variety of possibilities for binocular combination. By employing specially prepared stereoscopic cards in which the two monocular patterns are displaced in relation to one another by an amount equal and opposite to L's angle of squint, one can study the combination of the two "normal" images. Using the ordinary stereoscopic cards, one may attempt a combination of the "normal" image of one eye with the "anomalous" image of the other. Finally, one can employ a third set of patterns which are so arranged that "anomalous" images of opposite eyes can be combined.

In the experiments to be described in this section, two kinds of combination were attempted: that of "normal" and "normal" images, and that of "normal" and "anomalous" images. In both cases the results were the same.

It is well known that many squinters do not display the involuntary *fusion-movements* of the eyes which normally occur under disparate stimulation of the two retinae. In fact, according to Worth (19), the condition of squint is itself due to a weakness of this motor fusion-tendency, coexisting with a muscular imbalance (heterophoria) of the two eyes. Bielschowsky (4, p. 1102) also holds that deficiency of the motor fusion-tendency is a factor of major importance in the causation of squint.

In our subject L, this type of eye-movement is completely absent. If the normal subject looks, in the stereoscope, at two objects (e.g., dots or lines) which have a small disparity (horizontal, vertical or oblique), the eyes quickly and involuntarily move so as to bring the two objects together. If the disparity between the objects is not too small, the subject can observe their apparent movement towards one another. With a very small disparity, the fusion-movement often occurs so rapidly that the subject is not even aware that two separate objects were presented (and yet appropriate tests show that

an eye-movement has occurred). But in L's case, the disparity of the stimuli may be indefinitely small, and still they remain separate and motionless for him.

Although L lacks these involuntary fusion-movements, he displays the normal convergence-reaction which is associated with accommodation, and can control his convergence voluntarily. Thus he can, if he wishes, cause a pair of disparate stimuli to fuse by deliberately varying his convergence. As Bielschowsky (4, p. 1098 f.) has emphasized, the involuntary eye-movements of fusion are quite different in character from the convergence-movements that are linked with accommodation, and have a distinct neurological mechanism.

In the normal person, there is, in addition to this dynamic fusion, a static fusion of disparate objects which occurs without eye-movement. Single vision is not limited to strictly corresponding points, but occurs also when the opposite retinae are simultaneously stimulated by slightly disparate patterns of similar form. The subject L lacks not only the dynamic but the static type of fusion of disparate stimuli. He was shown a variety of stereoscopic patterns—e.g., two slightly different views of a truncated cone, two pairs of dots separated by slightly different intervals, and two lines crossing one another at a very small angle. So long as there was any disparity, no matter how small, between such patterns, they did not fuse for L. Binocular single vision, in his case, is limited to strictly corresponding points.

Like most other squinters, L obtains no depth effect in the stereoscope. When two disparate images of the "normal" type are superposed, he sees simply a sharp plane image with doubled contours. When a superposition of the "normal" image of one eye on the "anomalous" image of the other eye is attempted, the latter image is inhibited, unless the disparity is fairly large. If both are visible simultaneously, the anomalous image appears as a fuzzy and confusing halo bordering the contours of the normal image.

V

There is still another phenomenon of binocular "fusion" which the normal person displays: the fusion or mixture of different *colors* which are presented to corresponding areas of the two eyes.

L's binocular color-phenomena were studied by means of the

stereoscope, in which differently colored patterns made with colored ink or colored paper were presented to opposite eyes. In other experiments, colored gelatine filters of dissimilar hue were placed in front of the two eyes, and two white areas were so presented that their differently colored images were superposed in L's visual field.

The results to be described refer to the combination of the normal images of opposite eyes. The same general sort of result was obtained with the combination of the two anomalous images. (Of course, as we have already noted, the anomalous images do not give good chromatic colors.)

(When a superposition of *normal* and *anomalous* images of opposite eyes was attempted, phenomena of *inhibition* were encountered. If both images were small, compact figures, the anomalous image was inhibited by the normal image. This was the case even though conditions were so arranged that the anomalous image was much brighter than the normal image.

But if the anomalous image was a small, well-contoured figure, and the normal image consisted simply of a large, homogeneous background, both images could be seen simultaneously. Here again the results of combination conform to the description given below.)

In every experiment, L reported a singular and unexpected phenomenon. According to him, the two colors never fuse. Also, they never display alternation or rivalry, or appear as a parti-colored mosaic; neither is the one color seen as lying behind the other. Instead, he declares that *both colors are seen simultaneously in exactly the same place.*

L is fully aware of the seeming paradox involved in this statement, but firmly insists that it precisely describes the phenomenon. He is familiar with the fact that, under certain circumstances, one may have the experience of seeing one color through another color which appears to lie in front of it. In the present case, however, he maintains that both colors lie in the same plane, and occupy a common area in that plane, each of them completely filling the area.

This same report is given when the two monocular colors are white and black, or complementary hues like red and blue-green.

It is interesting to find that a phenomenon exactly like that which L reports can, according to Henning (6), be experienced by normal subjects under certain special conditions. Henning describes this effect as an *"optisches Ineinander,"* in contradistinction

to the *"optisches Hintereinander"* or appearance of two colors as lying in the same visual direction but at different depths. In most of his experiences on the *optisches Ineinander,* Henning employed a pair of outline drawings of different color.

> Sieht man mit dem linken Auge eine rote Figur, mit dem rechten Auge eine blaue Figur, die jede für sich mit starker Aufmerksamkeit zusammengefasst wird, so können die *Konturen des verschmolzenen räumlichen Bildes beide Farben am selben Orte des gemeinschaftlichen Gesichtsfeldes zugleich* haben, hier also sowohl rot, als blau. Weder liegt dann ein räumliches Nebeneinander verschiedener Farben oder ein farbiges Mosaik, noch ein Hindurchschimmern der einen Farbe durch die andere vor, sondern die Linien sind gleichzeitig überall am selben Orte sowohl rot als blau (6, p. 154 f.).

The same effect was obtained with complementary colors.

Henning states that the effect depends, first, on a very precise superposition of the two monocular contours, and second, on the attention. The figures must be small enough so that they can very easily be apprehended as wholes by simultaneous attention. The effect is easier to obtain with outline figures than with patterns of solid color; it also comes more readily with disparate figures than with identical figures. Not all subjects can obtain the effect.

But the reports given by our subject L would indicate that he does not require these special conditions in order to experience the Henning effect. For him it is the characteristic phenomenon rather than the exception.

The absence of binocular color-fusion, as well as the absence of fusion of disparate images, implies a remarkable degree of mutual independence between the two binocular fields. From several points of view, L's eyes seem to behave as two independent organs, rather than as two members of a functional unity. But the following experiment seems to show that a slight amount of interaction can take place between the two perceptual fields. By means of the stereoscope, a small white rectangle on a black ground was presented to one eye, and a larger white rectangle, also on a black ground, to the corresponding region of the other eye. Thus, there was a small area of white, perceived binocularly, which was surrounded by a region in which white and black appeared together in the same place (Henning phenomenon). According to L, the

white in the central region appeared a little lighter than the white of the adjoining region. When the experiment was repeated with a reversal of the figure and background colors, L reported that the binocular black appeared slightly darker than the adjoining monocular black. (This modification of a monocular color by the color seen with the opposite eye finds its analogy in the rivalry-phenomena of the normal subject. In binocular rivalry, there is seldom, if ever, a complete dominance of one monocular color; the dominating color shows a slight admixture of the color presented to the opposite eye.[6]) But in similar experiments with chromatic colors like red and green, L could detect no analogous differences between binocular and monocular colors.

The inhibition of *anomalous* images by the normal images of the opposite eye means, of course, that the two monocular fields are not completely indifferent to one another.

Experiments to test for *monocular* color-mixture between the normal and anomalous images were also performed, in analogy to those carried out by Bielschowsky. Unlike Bielschowsky's patient, L did not obtain any phenomena of fusion or rivalry between normal and anomalous monocular images of unlike color. Instead, he reported exactly the same appearance as in the binocular experiments: the two different colors were seen simultaneously in complete coincidence with one another.

In order to account for the Henning phenomenon, Müller (11, p. 108 f.; 12, p. 262 ff.) assumes that there is a separate central-nervous field (*Sonderfeld*) for each eye, in addition to the "mixture-field" (*Mischfeld*) which is the seat of binocular fusion and rivalry.

> . . . Das . . . Henningsche Phänomen beruht darauf, dass die von einem Netzhautpunkte ausgehende Erregung von einer zentralen Stelle ab nicht bloss eine M-Bahn betreten kann, die zu einem Mischfelde führt, das der Sitz der binokularen Farbenmischung bezw. des Wettstreites der Sehfelder ist, sondern auch auf eine S-Bahn übergehen kann, die zu einem Sonderfelde führt, in dem die Erregung unbeeinflüsst von einer Erregung des anderen Auges auf das Bewusstsein wirkt. (11, p. 109.)

[6]A different interpretation of the phenomena experienced by L in our experiment would follow from the ideas expressed by Müller (12, p. 265) in his discussion of the Henning phenomenon. The difference in appearance between the monocular and binocular colors would be due to a psychical summation of sensation-intensities in the binocular case.

Our subject L experiences the Henning phenomenon not only in binocular but in monocular vision. Therefore, if we accept Müller's assumption, we have to postulate that in L's case there are *two* independent fields (*Sonderfelder*) for each of the eyes—while the binocular mixture-field is out of function.

VI

A few general remarks will be made by way of conclusion.

We have noted above that L speaks of the anomalous image as having an "unreal" character. It may be suggested that this image owes its "unreality" largely to the fact that it shares the same locus in visual space with some portion of the "normal" field of perception. This of course is in contradiction to "reality." The normal image (of any well-contoured figure), on the other hand, inhibits the portion of the "anomalous" field which would otherwise occupy the same apparent locus. It is quite natural, then, that the one image should be called "unreal," the other "real."

In the case of our subject L, it seems quite clear that the anomalous images have no functional value whatever. They play no rôle in the subject's visual perception except that of an occasional minor distraction.

It is hardly necessary to remark that the case of L furnishes very positive support for the notion, already soundly established, that the normal correspondence of the retinae is congenitally determined. At the same time, in the binocular color-phenomena we have additional evidence in refutation of the hypothesis (already discredited) that the normal correspondence is based upon an anatomical fusion of the conducting paths leading from corresponding points.

VII

Summary

1. This report describes the visual phenomena of a squinter who possesses, in each eye, an acquired or anomalous system of localization in addition to the normal system. In consequence, he experiences diplopia not only in binocular vision but in vision with either eye alone.

2. This monocularly diplopic vision probably originated at a very early period in the subject's life. The existence of two anom-

alous systems, rather than merely one, seems to be connected with the absence of pronounced ocular dominance. The anomalous systems are devoid of any functional value.

3. There is a complete absence of the normal binocular phenomena of dynamic and static fusion of disparate images.

4. In place of binocular color-mixture, the subject reports a perception of two colors as occupying the same place at the same time (Henning phenomenon). This same description is given in the case of the monocular combination of "normal" and "anomalous" images of dissimilar color.

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University of Maine
Orono, Maine

LA DIPLOPIE DOUBLE MONOCULAIRE

(Résumé)

Ce rapport décrit les phénomènes visuels d'une personne louche qui possède dans chaque oeil un système acquis ou "anormal" de localisation outre le système normal. Par conséquent, elle subit la diplopie non seulement dans la vision binoculaire mais aussi dans la vision avec chaque oeil seul. Cette vision diplopique monoculaire a eu son origine probablement de très bonne heurs dans la vie du sujet. L'existence de deux systèmes anormaux, plutôt qu'un système seulement, semble avoir une relation avec l'absence d'une dominance oculaire prononcée. Les systèmes anormaux n'ont aucune valeur fonctionnelle. Il y a une absence complète des phénomènes binoculaires normaux de la fusion dynamique et statique des images disparates. Au lieu du mélange binoculaire de couleurs, le sujet rapporte une perception de deux couleurs qui occupent le même lieu en même temps (phénomène de Henning). Il donne là même description au cas de la combinaison monoculaire des images "normales" et "anormales" de couleurs dissemblables.

PURDY

DOPPELEINÄUGIGE DIPLOPIE

(Referat)

Dieser Bericht beschreibt die optischen Erscheinungen eines Schielenden, der in jedem Auge ein erworbenes oder "anormales" System der Lokalisierung zusammen mit dem normalen System besitz. Infolgedessen erfährt er Diplopie nicht nur im zweiäugigen Sehen, sondern auch im Sehen mit jedem Auge allein. Diese einäugige Diplopie enstand wohl zu einer frühen Zeit in seinem Leben. Das Bestehen Zwei anormaler Systeme anstatt nur eins scheint mit der Abwesenheit einer ausgesprochenen Augenvorherrschung verbunden zu sein. Die anormalen Systeme ermangeln irgendeines funktionellen Wertes. Es gibt eine völlige Abwesenheit der normalen zweiäugigen Phänomene der dynamischen und statischen Verschmelzung der disparaten Vorstellungen. Anstatt zweiäugiger Farbenmischung berichtet die Versuchsperson über das Wahrnehmen von zwei Farben, die denselben Platz zur selben Zeit einnehmen (Henning Phänomen). Dieselbe Beschreibung erfolgt in dem Fall der einäugigen Verbindung der "normalen" und "anormalen" Vorstellung verschiedenartiger Farbe.

PURDY

THE EFFECTS OF MUSICAL RHYTHMS AND PURE RHYTHMS ON BODILY SWAY*

From the Psychological Laboratories of the University of Wisconsin

RICHARD W. HUSBAND

I. PROBLEM

When a person stands upright it may be seen that he constantly sways slightly back and forth and from side to side. This is due to changes in muscular tension and tonicity, and possibly to external and internal stimulation. Sway is unconscious and automatic, and is kept within bounds by sensations aroused in the semi-circular canals, and by sensations of muscular strain. It is not abnormal in any sense, although it is exaggerated by extreme fatigue, drug action, or spinal lesion.

Miniature bodily movements are initiated by various situations which have emotional as well as motor components. It is a frequent observation that those watching a man high-jump at a track meet raise one leg as he springs, as if to help him along. Spectators at boxing matches deliver blows of their own at tense moments.

At a dance persons who are not participating at the moment often keep time with their feet or fingers, clap their hands, or sway with the rhythm. The affective side of this process has been given the term *empathy*.

It was this last observation, movements to music, which suggested this investigation. Our general problem is to study possible effects of music on body sway. Specifically, the chief problems were: (1) Does music cause a greater amount of sway than occurs under conditions of standing in silence? (2) Will different rhythms, march, waltz, or jazz, produce different amounts of sway? (3) Will their qualitative aspects be different? (4) Will pure rhythms, devoid of melody and harmony, produce similar sways?

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II. Apparatus

In both parts of this investigation we used the Miles Ataxiameter (2) as our measuring instrument. The apparatus was first used in a study of the effects of alcohol on postural stability. It records body sway quantitatively, while the subject is in a standing position.

A radio head set, with felt pads substituted for the receivers, is placed on the subject's head. A prong which sticks up from the cross-piece over the top of the head communicates to the recording mechanism any movement the subject makes by the following means. From a collar into which this prong is inserted threads lead to the recording wheels suspended from the corners of a square frame which surrounds the subject at a height slightly above his head. These wheels, or adders, are very light and free running, so the threads turn them with the slightest movement on the part of the subject. Pawls allow the wheels to rotate in one direction only, so that the total amount of sway adds up, without subtracting, when the subject returns to his original posture. Thus sway and recovery are recorded twice, both front and back, or right and left. Movements out of the four primary directions are communicated to two wheels in varying proportions, as might be expected. A second set of pawls keeps the wheels from moving at all before the actual test periods.

III. Technique[1]

In the two experiments we used a total of 156 university students as subjects. There were the usual variations in height, weight, and age. For the most part two subjects took turns in the apparatus during an hour of testing. This served to allow rest, although fatigue is an inconsequential factor in the short periods used; and to let the subject not being tested help the experimenter carry on one or two routine functions.

In general experimentation was carried on between 10 and 4:30, although there is no particular reason why this factor should make any difference. The apparatus and nature of the problem were briefly described to the subjects when they came into the room. They were advised to stand relaxed, it being pointed out that trying to resist sway only serves to tense the muscles and actually to increase

[1]The writer wishes to thank Miss Maxine S. Brostrom for assisting in collecting part of the data.

the sway. The subjects were told to stand with their heels together and their toes pointed slightly outward. Each test period was two minutes in length. In Experiment I each subject did six tests; in Experiment II each did five.

In brief, the two experiments were conducted as follows. In the first we presented the subjects with various musical passages in march, waltz, symphony, and jazz rhythms, as well as a control test of standing in silence. The second experiment eliminated the factors of melody and harmony and used only pure rhythmical stimuli. The subjects were presented with march, waltz, tango, and jazz rhythms sounded on a buzzer.

The details of the technique and apparatus will be discussed along with the results of the two experiments.

IV. Experiment I.

1. *Technique.* Thirty-eight men and 47 women were tested under each of these six conditions. (1) Standing with the eyes open; and (2) standing with the eyes closed; both in silence. (3-6) Four different types of music were played on a phonograph while the subject stood with his eyes closed. (3) March, Elgar's Pomp and Circumstance. (4) Waltz, Sibelius' Valse Triste. (5) Symphony, Rimsky-Korsakoff, Scheherazade Suite, 3rd movement. (6) Jazz, Wedding of the Painted Doll. Tests were administered in random order to even up possible systematic influences of practice, adaptation, fatigue, and boredom.

2. *Results.* The total amounts of sway for the men and women tested are presented in Tables 1 and 2. Separate presentation is given, since there are differences in height, and since there might possibly be qualitative differences in susceptibility to musical stimulation.

TABLE 1

Totals of Body Sway to Different Types of Music, for Men

Condition	Mean	σ_m	Range
1. Eyes open; silence	526	22	280-1019
2. Eyes closed; silence	746	37	359-1355
3. March	806	45	329-1745
4. Waltz	900	73	280-2330
5. Symphony	861	75	182-1305
6. Jazz	1023	74	528-1390

TABLE 2
TOTALS OF BODY SWAY TO DIFFERENT TYPES OF MUSIC, FOR WOMEN

Condition	Mean	σ_m	Range
1. Eyes open; silence	574	35	262-1090
2. Eyes closed; silence	702	37	410-2090
3. March	801	38	338-2090
4. Waltz	940	49	288-1680
5. Symphony	831	53	360-2352
6. Jazz	956	53	410-2130

The most important finding is that sway is greater to all types of music than when the subject is standing in silence. With the eyes open sway is somewhat diminished, apparently because one can detect slight displacement of objects in the environment visually before any motion in the muscles, joints, or semi-circular canals is appreciated.

Jazz music produces considerably more sway than other types of rhythmic stimulation. Apart from this the differences between the various types of music do not seem to be great. Part of this lack of any evident trends may be due to the tremendous individual variations. This makes the standard errors of the mean very large, in spite of the fact that the size of our groups is rather large for this type of experimentation.

3. *Discussion.* It seems quite likely that the great amount of sway produced by jazz music was due to its violent character and to the fact that the subjects are used to expressing themselves in movement to this sort of music. The waltz, another dance form, although not one in general use at the present time, produced the second greatest amount of sway. This is more pronounced in women than in men, which is perhaps in line with experience, since many girls study aesthetic dancing. It was rather surprising that the march rhythm caused less sway than any other form of music. This may have been due to the character of the piece, which was an orchestral arrangement. To test this hypothesis we put twenty-one women and seventeen men through the same experiment, except that they listened to a military band playing Sousa's Stars and Stripes Forever march. The women showed very slightly greater sway, while the men had a score almost as high as to jazz music. Again this might be caused by experience, as men generally have more marching practice than women.

We made no attempt to study the effects of experience on the part of the subjects. Practically none had had any special training along dance or marching lines. This might be made a subject for future investigation. Practically all may be assumed to have had considerable experience in social dancing, although not of a formal or trained nature.

The rank order is the same for both sexes. In general women swayed somewhat less than men, but this difference is at least balanced by their lesser height. Women might be expected to sway more than men, both because of experience and because of their feeling about the matter. Dancing as an activity seems to appeal more to women than to men, and they engage in it more, dancing among themselves, for example.

The qualitative as well as quantitative aspects of sway should be illuminating. It is conceivable, for example, that march music might produce sway largely in a forward-backward direction, while the movements to a waltz rhythm might be more circular in form, corresponding to the overt responses which might be made to these stimuli. Complete study of this would necessitate making a tracing record from the head as it moves. There are several practical difficulties in the way of recording and interpreting such movements, so we did not attempt it.

To see if some trends along this line could be detected, we compared the ratio of front and back sway with that to the right and left. We quote the coefficients in Table 3. A figure of 1.00 would

TABLE 3

RATIO OF LATERAL TO FORWARD AND BACKWARD SWAY

Condition	Men	Women
1. Eyes open; silence	.76	.87
2. Eyes closed; silence	.76	.80
3. March	.87	.90
4. Waltz	.89	.93
5. Symphony	.87	.83
6. Jazz	.92	.95

mean that the subjects swayed as much to the right and left as forward and backward, in general a complete circle. One of .50 would show a lateral sway of only half the extent of that to the front and rear. We see that with a single exception there is more

lateral sway to music than when the subject stood in silence. The highest coefficients are to the dance rhythms, both jazz and waltz. This is also in line with past experience in giving explicit expression to these types of music in the past. March music does not show a predominant forward-backward ratio, as might be expected from the usual use of such music. With a single exception the women show a higher ratio of lateral sway than do the men. These figures are only for the total amounts of sway and do not show any temporal relations.

V. Experiment II.

1. *Technique.* After concluding Experiment I it seemed possible that the results may have been due to melody, volume, speed, and varying emphasis, rather than to the rhythm alone. It is the unusual rhythm which makes tunes like La Paloma or Dvorak's Humoresque so catchy. It seemed very possible that additional sway might occur to the same piece, with only volume addition.

So in this experiment we decided to present to the subjects pure rhythmical stimuli. With the advice of Professor O. S. Dalley of the University of Wisconsin School of Music four well-marked rhythmical patterns were selected. These patterns were presented by a single-toned high-frequency buzzer. The rhythms chosen were march, waltz, tango, and jazz. The tango was substituted for the symphony used in the previous experiment because it has a much more definite pattern. A symphony may have movements in march, waltz, minuet, funeral, and other times, so one can not select any definite pattern as characteristic of this type of music. By giving the stimuli through buzzer sounds we ruled out all aspects of melody, harmony, volume, and other inflections.

In all other ways the experimental conditions and records taken were the same as in Experiment I. In addition to the four experimental situations we obtained a control record of each subject standing in silence. Eyes were closed in all tests. We omitted from this part the test with the eyes open, as it did not seem necessary to duplicate it.

2. *Apparatus.* The various rhythmical patterns were presented by a high-frequency buzzer. To present these uniformly we found it necessary to devise a special apparatus. A kymograph drum was covered with paper which had been cut out in a manner similar to

player piano rolls. Where the strips had been cut out metal styli came into contact with the drum. Where the paper remained, it acted as an insulator. Thus the buzzer sounded in accordance with the pattern which had been cut out.

The drum was rotated by means of a ¼-horsepower, 1725-R. P. M., 60-cycle A. C. motor. A worm gear on the shaft of this drove a very large cog wheel so that the speed was reduced to make one revolution of the drum in between six and seven seconds. Since this motor made considerable noise it was placed in another room across the corridor. A set of switches in the testing room enabled the experimenter to start the motor and to select any one of the four rhythmic patterns. The 500-cycle buzzer was attached to the ataxiameter frame and sounded about two feet from the subject's head.

3. *Results.* The amounts of sway produced by the various pure rhythms are given in Tables 4 and 5. We see again that stimulation

TABLE 4

TOTALS OF BODY SWAY TO DIFFERENT PURE RHYTHMS, FOR WOMEN

Condition	Mean	σ_m	Range
1. Silence (control)	558	20	275- 825
2. March	676	19	312- 946
3. Waltz	700	20	399-1069
4. Tango	683	22	347-1071
5. Jazz	679	21	292- 959

TABLE 5

TOTALS OF BODY SWAY TO DIFFERENT PURE RHYTHMS, FOR MEN

Condition	Mean	σ_m	Range
1. Silence	644	18	397- 893
2. March	710	16	512- 897
3. Waltz	706	17	399- 956
4. Tango	746	17	442-1079
5. Jazz	750	18	398-1133

produces more sway than standing in silence, but that the pure rhythms increase instability by only approximately a third the extent caused by the complete pattern of meaningful music. The averages for the different rhythms are so close to one another, slightly over one standard error of the mean at the most, that no significant differences

or even reasonably reliable trends can be pointed out. It is to be noticed, furthermore, that the huge variations which occurred in Experiment I are reduced to a fraction of their former extent.

VI. Summary

1. To test experimentally the observed fact that people sway to music, we measured the body sway of subjects standing in the ataxiameter under two different conditions. Thirty-eight men and 47 women were presented the following stimuli: standing in silence, both with eyes open and closed; and with the eyes closed to these four types of music: march, waltz, symphony, and jazz. Next, 36 men and 35 women were tested to pure rhythms, automatically sounded on a high frequency buzzer. The typical rhythms used were waltz, march, tango, and jazz, as well as a silent control period.

2. All types of music produced more sway than when the subject stood in silence.

3. Pure rhythmical stimuli caused an increase over normal sway while standing in silence only about one-third as much as that produced by actual music.

4. Jazz caused the greatest amount of sway. This seemed to be produced both by its violence and by the experience the subjects have had in the past in responding to it. Among the pure rhythm patterns there was little differentiation, beyond a bare suggestion that speed caused more sway. There were no marked differences among the symphony, waltz, and march times.

5. The ratio of lateral to frontal sway was greater with the two dance rhythms than with march or symphony music.

6. There were practically no sex differences. Men showed somewhat greater sway, which is practically entirely attributable to variations in height, but the rank orders to the various stimulating conditions were virtually the same.

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University of Wisconsin
Madison, Wisconsin

LES EFFETS DES RYTHMES MUSICAUX ET DES RYTHMES PURS SUR LE BALANCEMENT DU CORPS

(Résumé)

Les mouvements du corps sont souvent imités par la musique, un désir fort, etc. On a étudié l'influence de divers types de musique sur le balancement du corps, en employant des rythmes de marche, de waltz, de symphonie et de jazz. Dans la première expérience on a employé la musique phonographique; dans la seconde on a présenté les mêmes rythmes au moyen de timbres électriques, éliminant ainsi la mélodie et l'harmonie. Tous les types de musique ont causé un balancement plus fort que celui ayant lieu en silence. Le rythme du jazz a produit de beaucoup la plus grande augmentation du balancement, tandis que les trois autres types ont eu un effet à peu près égal. Les stimuli des timbres, qui n'ont possédé aucuns éléments musicaux sauf le rythme, n'ont ajouté qu'environ le tiers du balancement ajouté par la vraie musique au test en silence. Les divers rythmes ont été tous très proches les uns aux autres; les différences semblent explicables sur la base de la rapidité plutôt que par les rythmes eux-mêmes. Les hommes montrent un peu plus de balancement, probablement à cause de leur plus grande taille, mais autrement il n'y a aucunes différences spéciales entre les sexes dans l'influence des divers rythmes.

HUSBAND

DIE WIRKUNGEN DES MUSIKALISCHEN RHYTHMUS UND DES REINEN RHYTHMUS AUF DAS KÖRPERLICHE SCHWANKEN

(Referat)

Körperliche Bewegungen werden oft durch Musik, starke Lust, usw. nachgeahmt. Wir untersuchten den Einfluss der verschiedenen Arten von Musik auf das körperliche Schwanken, wie Marsch-, Symphonie- und Jazzrhythmen. In dem ersten Versuch gebrauchten wir Grammophonmusik; in dem zweiten wurden dieselben Rhythmen durch Tonapparate dargeboten; dabei sind Melodie und Harmonie ausgeschaltet. Alle Arten von Musik vermehrten das Schwanken über das in der Stille. Die Jazzrhythmen erzeugten bei weitem die grösste Vermehrung des Schwankens, während die anderen drei Arten ungefähr gleich in ihrer Wirkung waren. Die Tonapparatreize, welche musikalischer Elemente ausser Rhythmus ganz ermangelten, vermehrten um ungefähr ein Drittel mehr das Schwanken über den Versuch in der Stille als die wirkliche Musik. Die verschiedenen Rhythmen waren einander sehr nahe verwandt; alle Verschiedenheiten schienen erklärlich auf Grund der Schnelligkeit eher als der Rhythmen selbst. Die Männer zeigen etwas mehr Schwanken, wahrscheinlich weil sie grösser sind, aber sonst gibt es keine bedeutenden Unterschiede zwischen den Geschlechtern hinsichtlich des Einflusses verschiedenartiger Rhythmen.

HUSBAND

AN EXPERIMENTAL STUDY OF SPEECH PERCEPTION*

From the Department of Psychology of the University of Colorado

T. H. Howells and J. B. Schoolland

Deafness was one of the earliest subjects of psychological investigation. Individual differences in reaction to auditory stimuli have always been very apparent, and also quite susceptible to measurement. Very simple techniques were adequate to demonstrate these differences, e.g., the familiar watch-tick method. In recent times the methods of measuring auditory acuity have been greatly refined and extended and a great deal of highly specialized apparatus has been produced. Instead of determining the threshold for a random, complex sound, such as a watch-tick, the present practice tends to analyze out the elementary auditory stimuli and responses, to determine the sensitivity to relatively pure tones, and to map the profile of acuity throughout the whole pitch-range. The danger in this analytic procedure, as in all reductive processes, is that it may cause one to overlook or even deny any possible combination effects.[1]

Along with the improvement in techniques for measuring auditory acuity has come also an appreciation of its practical importance, so that now the diagnosis of deafness is a recognized routine in child-welfare and guidance work. Much money and time has been devoted to extensive surveys of deafness of children. It seems plain that sensitivity of hearing is widely recognized as a very important factor in the genesis of personality.

The Problem

So much attention has been given to auditory sensitivity or acuity that there is apparently a tendency to take it for granted that acuity

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[1]In an unpublished study T. H. Howells has obtained tentative evidence that the acuity threshold for certain pairs of tones is lower than for either one of them alone when the sound energy is held constant. This is implied also by the work of Wegel and Lane (12).

deficiency and auditory deficiency are one and the same thing, or in other words, that all auditory defects are acuity defects and that the terms can be used synonymously. It is a notable fact that the instruments most generally used for measuring acuity are termed audiometers. It is evident, of course, that this usage is not precise. Ear specialists are familiar with cases of word-deafness in which the patient is practically incapable of understanding the speech of those about him, although he can hear a clock tick and shows normal sensitivity when given an acuity test. It would be very much of a mistake to pronounce such hearing as normal on the basis of such a test, but the mistake has undoubtedly been made many times (3).

There are also less extreme deficiencies in hearing which are likewise not explainable in terms of deafness alone. Many people whose hearing is obviously keen enough have great difficulty in understanding speech at a talkie theatre, in a noisy factory, or at a social gathering where there is much conflicting sound. A telephone operator who was discharged because of inability to understand spoken numbers was found to be above normal on an acuity test. The fact that an increased volume of sound does not alleviate the difficulty seems to indicate that it is not due to insensitivity, but rather is the result of inability to select and organize the elements in the sounds heard, or in other words that it is due to imperception.

There is a surprising poverty of studies, either experimental or theoretical, on the subject of auditory perception. Stumpf (10), in his classic survey, discusses the problem of perception but outlines no experimental techniques. Troland (11) refers briefly to auditory perception as a problem in psychophysics. The problem is also raised by Ruckmick (7, 8) in his reviews of research in audition. Wiley (14) raises the problem of speech perception from a neurological point of view. Knudsen and Jones (4) compare perceptual and acuity explanations of paracusia, while Hall (3) cites cases of auditory imperception. The differential masking and emphasis effect of tones on each other is indicated by the work of Wegel and Lane (12). Wever (13) reviews evidence that a secondary sound affects the perceptual and acuity thresholds for other sounds. Meyer (5) denies the adequacy of explanations of audition in physical terms only. Clarke (1) observes the general inadequacy of hearing tests to diagnose hearing ability. The effect, in general, of artificial mutilation of speech, through changes in relative frequency content,

etc., has been observed by a number of people and perceptual difficulties studied. This work is described in detail by Fletcher (2). The problem of individual differences in perception of speech sounds or of the means of measuring them does not seem to have received attention.

The Measure of Speech Perception

In order to test the hypothesis of imperception as a hearing handicap a speech-perception test was devised which presented, through the medium of phonograph records, a series of speech sounds of varying difficulty in interpretation.[2] This test served as a means of classification of individuals with respect to their ability to understand or perceive spoken words. In addition it was necessary to measure differences in auditory acuity between these same individuals, and to determine the degree of correspondence between the perceptual and acuity factors.

The phonograph records were prepared by dictating 124 words into a recording phonograph, in much the same way that they would be pronounced before a spelling class. During this same time a barrage of conflicting noise was produced which was also recorded. This noise was obtained by shaking objects in wood boxes, by crumpling paper held near the microphone, by a confused background of speech, etc. While the interference was purposely made more intense for some of the words than for others, it was impossible, of course, to predetermine just what amount of interference there would be for any particular word. Once recorded, however, the interference remained the same always for each particular word. The general imperfection in recording and reproducing speech, which is ordinarily a fault, became a virtue in this case, since it increased the difficulty of the test. The general effect was to produce a suitable range of difficulty in recognizing the words when the records were played.

The words were familiar and unequivocal with equal proportions of one, two and three syllable forms, and arranged in random order. It should be understood that in the present instance the purpose was to provide a typical or representative variety of speech sounds and also of sound interferences. It was not the object of this study to select or limit the sound elements either in the stimulus words or in

[2]A similar technique was used by Fletcher (2) to measure the influence of frequency components etc. on intelligibility.

the conflicting sounds, since it was primarily an investigation of differences in individuals rather than situations as in the case of Fletcher (2). It was necessary, of course, that the sound situations be as uniform and as normal as possible. While no graphic analysis was attempted the recording was electrical and there were no obvious gaps in frequency components.

The test was given by playing the records in a small room before groups of university students of about fifteen each. They were arranged about equally distant from the phonograph. Outside noises were minimized and conditions were standardized in so far as possible. The volume of sound was purposely maintained at a uniformly loud level. The words had been dictated and were reproduced at four-second intervals. The subjects were asked to write down the words as heard on a form provided. The number of words correctly written, from the standpoint of sound, constituted the score. Scores were obtained for 110 subjects.

A satisfactory range of talent was obtained, approximating the normal curve of distribution. Two words only of the 124 were missed by everyone. One word was recognized by 97 per cent of the group, and two by 92 per cent. Seven words were heard once only. Of the 3700 recognitions, 26 per cent were of one syllable words, 22 per cent of two syllable words, and 52 per cent were of three syllable words. The mean score was 43 words. The highest score was 67 words and the lowest was seven, with a standard deviation of 13.2. The correlation of the scores on the odd-numbered words with the scores on the even-numbered words was .85, affording a coefficient of reliability, according to Brown's formula, of .92. It seems evident that the test was measuring some ability with considerable accuracy.

Speech Perception and Acuity

The next procedure was to measure the auditory acuity of the same subjects for the purpose of comparison. They were tested both by the watch-tick method and also with the Seashore electrical audiometer. The scores on each test were the result of averaging ten trial scores. These trial scores were obtained by combining the individual scores for both ears, allowing the better ear four times the weighting of the poorer one in the combination. The score for each ear was obtained by averaging the ascending and descending thresholds. In

the case of the watch-tick test the watch was mounted on a string which ran over pulleys with a scale between them, so that the distance of the watch could be varied silently and at the same rate. The coefficients of reliability, obtained by correlating the odd trials against the even trials and applying Brown's formula, was .94 for the watch-tick method, and .97 for the Seashore audiometer.

It is an interesting fact that the correlation between the scores on these two reliable acuity tests was only .41, indicating apparently that acuities are specifically related to specific stimuli, and are common, perhaps, only in so far as the stimulating elements are common.

There are no reliable norms, apparently, for either of these acuity tests. The watch-tick test obviously could not be standardized, except for a particular watch with a given adjustment in a specific laboratory situation. The range of acuity as measured by the Seashore audiometer seemed to be about normal according to norms that were available. There were, however, no subjects who were found to be extremely deaf according to either test. This would be expected, of course, since they had all been able to achieve college rank in a normal school system. This fact may indicate that the distribution would be somewhat more restricted than normal. There were, however, a few subjects who were almost completely deaf in one ear.

As was stated before one of the principal objectives of this study was to determine the part played by auditory acuity as affecting the efficiency of hearing in normal situations. The coefficient of correlation of the scores on the described test of speech perception with the scores on the Seashore acuity test was .08 for 110 cases, while with the watch-tick test the coefficient was .06. It seems apparent that, for the subjects used and for the intensity of the sound provided by the phonograph, variations in sensitivity or acuity did not appreciably affect efficiency in hearing.

Correspondence with Other Factors

An associated problem which arises naturally in connection with the study of speech perception is that of its correspondence with other types of perception. Scores were obtained for 38 of the subjects on a test of visual perception of printed letters and words.[3] This test consisted of letters and words of varying degrees of blurredness,

[3]Developed by T. H. Howells in an unpublished study.

which were photographically printed upon a series of charts by means of an enlarging lantern which was adjusted so that the images were more or less out of focus, much as in an imperfect eye. The aim was to hold distinctness comparatively constant for all the subjects and to classify them with respect to ability to interpret the blurred images. The fact that the problem consisted in organizing blurred imagery seems to justify the term, perception. This test showed a reliability coefficient of .86. The correlation with speech perception scores was .07.

Scores were also available for 44 of the subjects on a test of ability to recognize faces.[4] In this test three photographs of the faces of 42 different persons were grouped on a card, so that there were 42 cards with three portraits of a given individual on each. Corresponding groups of three different portraits of each of these persons were placed in random order on a large chart. Subjects taking the test looked at the face on a given card for ten seconds, laid it down, and then attempted to find the same individual from among the groups of portraits on the chart. The number of correct identifications constituted the score. The portraits on the chart differed in facial expression, angle of view, size, clothing etc., from the corresponding ones on the chart, which necessitated identification in terms of facial pattern, rather than through discrete common elements. Chance factors served to bring about a suitable range of difficulty. The observed reliability coefficient of this test for 134 cases was .88.

The fact that this test, in addition to measuring perceptual or organizing ability, was concerned with stimuli which were primarily social would seem to indicate that it might have much in common with the test of speech perception. There was a correlation of .14 between the scores on these tests for 44 cases, which would seem to suggest that this is not the case.

Since the tests just described deal with visual phenomena it might be presumed that there would be little correspondence with the speech-perception scores, and that auditory measures might correlate more highly. The Seashore Tests of Musical Aptitude (7) were given to an average of 80 of the subjects who had taken the speech-

[4]Preliminary report by T. H. Howells in the Journal of the Colorado-Wyoming Academy of Science for 1933.

perception test. The coefficients of correlation of the speech-perception scores on the various units of the Seashore test are presented in Table 1.

TABLE 1

CORRELATIONS WITH SPEECH-PERCEPTION SCORES (Rel. .92)

Test	Reliability	No. of cases	Coefficient	P.E.
Acuity (audiometer)	.97	110	.08	.064
Acuity (watch-tick)	.94	110	.06	.064
Facial memory	.88	44	.14	.102
Reading blurred print	.86	38	.07	.106
Spelling	.90	62	.05	.086
Intensity		75	.005	.078
Consonance		86	.03	.071
Rhythm		88	.06	.071
Pitch		92	.09	.070
Time sense		65	.12	.080
Tonal memory		69	.21	.077
Grades		100	.20	.065
Intelligence	.96	71	.37	.069

It is evident that the correspondence is very slight. Only in the case of tonal memory does it approach significance.

It has sometimes been suspected that deficiency in spelling ability may be due to a perceptual difficulty, either auditory or visual or both. A preliminary form of the speech-perception test was given to eighty-four subjects who were also given a spelling list of 92 words taken from a commercial spelling book. Reliabilities were not ascertained, but there was a coefficient of .35 between the measures. Sixty-two of the subjects taking the final form of the speech-perception test were given a list of 150 words selected at random from the freshman spelling list used at the University of Colorado. The reliability of the spelling scores was .90, and their correlation with the speech-perception scores was .05. It seems that there is little in common.

There was more correspondence with the more complex and inclusive functions of grade achievement and general intelligence. The correlation of speech-perception scores with a composite of grades achieved by subjects in university courses was .20 for 100 cases. The correlation with intelligence scores obtained from the American Council Test was .37 for 71 cases. When the intelligence factor is partialled out the correlation of speech perception scores with grades becomes .10.

The data on sex differences are not conclusive. The test was taken by 46 men and by 64 women. The mean score for the men was 40.9, while for the women it was 44.8. The difference between these means was 3.9, which is 2.3 times the probable error. The difference seems suggestive, but it is hardly significant.

Summary and Review

By way of summary it appears that:

1. The common assumption that ability to hear speech is analyzable into elementary factors, or acuity thresholds, is unwarranted on the basis of available evidence.
2. A test of speech-perception, involving the recognition of words pronounced by a phonograph, was constructed and administered to 110 subjects, and found reliable.
3. The speech-perception scores showed little correspondence with scores on two reliable tests of auditory acuity, indicating that the acuity factor in hearing has been overestimated.
4. There was little or no relationship between speech perception scores and the data from (1) a facial memory test, (2) a test of ability to read blurred print, (3) a spelling test, (4) the Seashore Tests of Musical Aptitude: namely, *consonance, rhythm, pitch-sense, time-sense,* and *tonal-memory.*
5. The correlation of speech-perception scores with composites of grades in university courses was .20, while with intelligence scores the coefficient was .37, indicating some elements in common in each case.
6. The data suggest, but do not prove, that women excel men in speech-perception.

The most significant outcome of the experiment seems to be the demonstration and measurement of a non-sensory, auditory function which has much to do with ability to understand conversation. Supporting this conclusion is the fact that ability to hear in the test-situation was not dependent upon auditory acuity as evidenced by a negligible correlation with two reliable acuity tests. The implication seems to be that acuity has been overestimated as a factor in hearing and that more attention should be paid to the auditory function as a totality. As a correlate of this review it is worthy of note that Reudiger (6) found similarly in the case of vision that acuity

has little to do with differences between individuals in visual perception.

It should be noted that the present study is of differences between individuals in reaction to a fairly normal pattern of auditory stimuli, while Fletcher's studies (2) were of differences in the pattern of stimuli as regards their influence upon the perception of the normal or average individual. Either, or various combinations of both techniques could be devised for the study of various problems, as, for example: (1) influence of the time (e.g., between words) allowed for the perception, (2) the influence of the amount or nature of context, or meaningful relationship, (3) the influence of different social settings when giving the test, (4) the relationship between perception and such factors as phase difference, frequency content, intensity, etc., (5) the effect of practice on ability to supply missing perceptual elements (subjective tones etc.), and of disuse or adaptation on the disappearance of these elements, or (6) of the differential effect of these elements on each other. With a properly graded series of tests, similar to the one described, it should be possible to study the genesis of speech-perception in the child, and to detect abnormality at an early stage.

The lack of inter-consistency between the various perceptual tests, especially when viewed in light of their high intra-consistency, seems to constitute one of the most perplexing problems of psychology. The evidence in this study is merely a supplement to that supplied by many others. There seems to be no common perceptual factor. Whatever slight intercorrelation there is can be explained, apparently, on the basis of a common permeating factor of intelligence.

The most obvious and practical implication of the study seems to be the need for a test, or series of tests, similar in general to the one described, which should be refined and thoroughly standardized, with different forms of the test and with different norms developed for various social and age-groups. Such a test could be provided in the form of records manufactured and distributed by one of the phonograph companies as in the case of the Seashore tests. It seems that such a test should prove of much value in the more exact diagnosis of hearing defects.

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University of Colorado
Boulder, Colorado

UNE ÉTUDE EXPÉRIMENTALE DE LA PERCEPTION DE LA PAROLE

(Résumé)

On croit ordinairement que cela va sans dire qu'un test d'acuité auditive ou de sensitivité est aussi un test de la capacité d'entendre les mots parlés. Il est évident, cependant, que la difficulté que l'on trouve ordinairement à distinguer les mots parlés à haute voix mais imparfaitement, ou dans une confusion d'autres sons, n'est pas due aux oreilles insensibles, mais à l'imperceptibilité.

On a fait un test qui se compose de stimuli ordinaires de la parole de difficulté variante, en dictant des mots ordinaires et en les enregistrant sur des disques phonographiques, avec les divers sons en conflit, aussi. On a fait jouer les disques d'une manière retentissante devant 110 sujets. Le numbre des mots compris a constitué le résultat. La constance de la

mesure a été de 0,92. On a trouvé des cas importants de l'imperceptibilité. La correspondance avec plusieurs autres mesures s'est montrée légère, sauf pour une corrélaton de 0,37 avec l'intelligence. Le résultat le plus signifiant a été une corrélation négligeable entre les résultats de la perception de la parole et ceux de deux tests d'acuité auditive statistiquement de valeur.

On conclut que l'on a beaucoup exagéré l'acuité auditive comme facteur de l'ouïe, et que l'on devrait faire plus d'attention à la fonction auditive comme totalité. On suggère que l'on devrait construire des tests semblables et les standardiser pour divers groupes sociaux. On pourrait se servir de tels tests dans les études de la philologie comparative, ainsi que dans le diagnostique des difficultés de l'ouïe.

HOWELLS ET SCHOOLLAND

EINE EXPERIMENTELLE UNTERSUCHUNG DER SPRACHWAHRNEHMUNG

(Referat)

Es wird allgemein angenommen, dass ein Merkmal der Gehörschärfe oder Empfindlichkeit auch ein Merkmal der Fähigkeit sei, gesprochene Worte zu hören. Es ist aber einleuchtend, dass die Schwierigkeit, die gewöhnlich bei der Unterscheidung von laut oder mangelhaft gesprochenen Worten oder bei einer Verwirrung von anderen Lauten entsteht, nicht die Folge unempfindlicher Ohren ist, sondern einer mangelhaften Wahrnehmung.

Eine Prüfung, die aus gewöhnlichen Sprachreizen von verschiedenartiger Schwierigkeit bestand, wurde durch das Diktieren von gewöhnlichen Worten und deren Aufzeichnung mit verschiedenen widerstreitenden Lauten auf Grammophonplatten hergestellt. Die Platten wurden vor 110 Vpn. vorgespielt. Die Anzahl der verstandenen Wörter ergab den Wert für die Prüfung. Die Zuverlässigkeit des Masses war 0,92. Hervorragende Fälle der mangelhaften Wahrnehmung wurden entdeckt. Die Uebereinstimmung mit einigen anderen Massen war gering, ausser einer Korrelation von 0,37 mit der Intelligenz. Der bedeutsamste Befund war eine geringfügige Korrelation zwischen den Sprachwahrnehmungswerten und den Werten von zwei statistisch zuverlässigen Prüfungen der Gehörschärfe.

Es wird gefolgert, dass die Gehörschärfe als ein Faktor des Hörens sehr überschätzt worden ist und dass mehr Aufmerksamkeit der Gehörfunktion als Ganzheit zugewandt werden sollte. Es wird vorgeschlagen, dass ähnliche Prüfungen aufgestellt und standardisiert bei verschiedenen Sozialgruppen werden sollten. Solche Prüfungen könnten auch in Studien der vergleichenden Philologie sowie in der Diagnose von Gehörschwierigkeiten verwandt werden.

HOWELLS UND SCHOOLLAND

A STUDY OF THE RELATIONSHIP BETWEEN CHANGES IN BREATHING AND PULSE RATE, AND THE AMOUNT LEARNED FOLLOWING SUPPOSED EMOTIONAL AND SUPPOSED NON-EMOTIONAL STIMULI*[1]

From the Department of Psychology of the University of Kentucky

LAWRENCE M. BAKER

THE GENERAL PROBLEM

The general problem in these experiments was to study the relationship between learning and changes in pulse and breathing when these changes were produced by supposedly emotional and supposedly non-emotional stimuli. In each condition the stimulus was given immediately before the learning period for the purpose of eliciting changes in the breathing rate and pulse rate.

The writer knows of no one who has made a comparison of the effects upon learning produced by a stimulus causing an emotional response and those elicited by a stimulus which is of a non-emotional nature when the physiological factors, such as pulse, blood-pressure, and breathing are similarly affected under the two conditions. The evidence on the effects of emotional stimuli upon learning has been limited and indirect, and not very definite conclusions have been drawn.

Numerous studies have been made upon the effects of affective tone upon memory. A few studies, more closely related to our problem, have been made upon the effect of interpolating an emotional stimulus between learning and the recall. Ordinarily, the physiological changes have not been recorded. This is a valuable addition as it probably gives an indication of the affectiveness of the stimulus. In the present study the stimuli were applied before the learning began. It is probable, therefore, that they affected more directly the learning

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[1]A thesis submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy at the University of Kentucky. The writer wishes to express his appreciation to Dr. M. M. White who directed the work.

process than the retention of the material learned. This is an important difference from those studies where the stimuli were interpolated after the learning period and before recall. In such cases it is the effect upon retention which was studied. In our experiment it is the learning or memorizing which is primarily affected, though possibly also the retention and recall to some extent.

There are two possible methods of approaching the problem in studying the relation between physiological changes and changes in learning. One is to determine what physiological changes take place when the learning is in process. Gillespie (10) used this method when performing an experiment upon the relative influence of mental and muscular work upon the pulse rate and blood-pressure. He gave the subjects work to do and recorded the pulse and blood-pressure while they were doing it. The other approach is to give the subject a stimulus which will change the physiological functions and observe the effects upon the subsequent learning. This method was used in the present study since it enabled us to compare the results in what we have considered as emotional and non-emotional situations. However, the experiment was planned so that the influence of learning upon the physiological functions could be studied as well as the influence of the physiological changes upon learning. When the pulse rate and breathing rate in the control period, in which there was learning with no other stimulus, were compared with the normal rates we have an example of the first method. When the learning in the experimental period was compared with the learning in the control period we have an example of the second type of procedure, for then an attempt was made to determine the effects of the physiological changes upon the learning. This procedure is similar to that used by Bills (2) when he had the subject squeeze a dynamometer in order to increase the muscular tension during the learning period. He did not record the pulse or breathing rates in his study.

A solution of the problem, as we have stated it, should have important bearings upon factors which may facilitate or inhibit learning under different circumstances. If the amount of learning under different conditions varies, an attempt to explain this fact might be made by consideration of the nutritional factors involved, such as changes in blood supply and oxygen consumption, or by the influence of muscular tension and relaxation upon the organism.

The experimental work of Bills (2), Stroud (17), and Freeman (8) shows rather consistently that learning is increased if it is done while the muscles are more tense than normally. It is not known whether this improved learning results from the increased muscular tonicity or whether it is due to some other factor and is only an accompaniment of the tension.

Experiment I

Purpose and Procedure. The problem in this experiment was to determine the effect of an alleged emotional stimulus upon learning, and to determine the relationship between the changes in learning and pulse rate.

The subjects were forty-six college students taken from the laboratory sections of the beginning psychology classes at the University of Kentucky. The material presented to be learned consisted of two lists of one-syllable words. By using simple words it was believed that more cooperation was obtained than would have been with nonsense syllables for words are usually more interesting, and they are also more like the material students learn in ordinary school work.

The words were shown by means of an auto-card flash machine, and were presented in the same order to each subject so that any advantage due to position of words would be constant. After each list was presented the subject was given a book to read for two minutes. The subjects began reading at the same passage and the material to be read was descriptive and supposedly non-emotional in nature. This period was given to reduce the effects that an error of a few seconds in the interval might have upon the recall, and some time was necessary for stopping the apparatus and giving the subject writing material. The reading was to minimize reviewing. After the first list had been written the subjects were given five minutes to relax and were told that they would not be given any more about those words. This interval was designed to eliminate any confusion between words in the two lists that might arise in recalling the second list.

To get what we have considered as an emotional stimulus the chair in which the subject was sitting was dropped about fifteen inches. The writer is indebted to Blatz (4) for the idea of using a falling chair to get an emotional stimulus. Blatz found that the

subjects in his experiment usually described the experience as fear. Our technique differed from his in that the chair fell a shorter distance.

A record of the pulse rate was obtained by means of a sphymograph, attached about the arm, which had a rubber tube leading to a tambour in the adjacent room. A lever was attached to the tambour and the lever wrote on a revolving smoked drum.

When the subjects were seated in the chair and the apparatus properly attached, they were instructed as follows:

> This is a problem in learning. The sphymograph is used to take the pulse rate while you attempt to learn some words. There are fifteen words in the list to be learned. Each card has one word on it and the words will be presented but once. Later you will be asked to write the words that you can recall. This is not an intelligence test. Lean back in your chair comfortably but try not to move your arm as it will interfere with the recording.

To avoid the possible effects of practice the control period of learning was alternated with the experimental period. In order to control the influence which a difference in the difficulty of the words in the two lists might have, each was given half of the time for the control and half for the experimental period. The number of words recalled was the criterion of the amount of learning that had taken place. Only those words that were correctly spelled were counted.

As the first experiment was somewhat exploratory in nature, the 46 subjects were divided into two groups. The first 25 were given the learning to do immediately after the emotional stimulus, the other 21 were not given the words until two minutes after the chair had fallen.

Results. The results of the subjects who learned the words immediately after the stimulus are given in Table 1. In this table we see that there were only 4 subjects out of the 25 who learned fewer words after the stimulus or shock than in the control period. When the total words learned by all the subjects were combined and a comparison made between the control and experimental periods, it was found that 139 words were recalled for the experimental, or an average of 5.56 words for each subject. The total recalled for the control period was 114 words, or an average of 4.56 words for each subject. This is an improvement of 22 per cent in the experimental

TABLE 1

THE NUMBER OF WORDS THAT WERE LEARNED IMMEDIATELY AFTER SHOCK, AND CHANGE OF PULSE RATE

Subjects	After shock words	Without shock words	Pulse change per min.	Diff. words
1	6	5	0	1
2	6	3	6	3
3	7	5	3	2
4	4	3	5	1
5	2	4	3	—2
6	4	2	0	2
7	5	3	4	2
8	8	5	12	3
9	4	4	—4	0
10	6	3	0	3
11	5	4	12	1
12	5	5	12	0
13	6	7	0	—1
14	4	1	0	3
15	3	6	9	—3
16	6	5	4	3
18	5	5	0	0
19	4	5	—6	—1
20	9	8	15	1
21	7	4	0	3
22	8	8	4	0
23	6	5	0	1
24	3	2	0	1
25	8	8	4	0
Total	139	114	83	25

After shock: Experimental period; number of words learned after the chair fell.
Without shock: Control period; words learned where no shock was given.
Pulse change: Changes of pulse following the shock.
Diff. words: Difference in number of words learned in experimental and control periods.
Negative numbers indicate decreases in experimental period.

period over the control. The difference in the averages is reasonably reliable statistically, as the chances are about 98 in a hundred that there is a true difference in favor of the experimental period.

It was thought that the relationship between the number of words recalled and the changes in pulse rate might be significant and that this might afford some explanation of the greater learning

in the experimental period and also of the individual differences. There was no marked change in the pulse rate for most of the subjects, but in all the subjects except two where there was any change following the fall of the chair the rate was greater.

The results from the 21 subjects who waited 2 minutes after the stimulus before the words were presented indicated that practically all the effects of the emotional stimulus had disappeared. A table of the results has not been reproduced here. However, 10 subjects learned more in the experimental period, 8 fewer, and 3 the same number of words as in the control period. The total number of words learned by all the subjects in the two conditions was the same —113 in each case. The pulse had returned to normal for most of the subjects. There was a slight tendency for the rate to be slower than in the control period which may indicate a tendency toward compensation for the increased rate immediately after the the chair fell.

Summary. Our problem was to determine whether an alleged emotional stimulus affects learning, and to see what the relationship was between changes in pulse rate and learning. A falling chair in which the subject was seated was used as the emotional stimulus for the experimental period. Records of the pulse were made during both the control and experimental period of learning. We may conclude that immediately following a stimulus of this type and intensity learning was increased for most of the subjects used. Likewise the pulse rate was increased for most of the subjects, which may suggest a positive relationship between changes in pulse rate and changes in learning. The stimulus apparently had little or no effect upon learning or pulse rate at the end of a two-minute interval.

This experiment may be considered as somewhat of a preliminary study. The results seemed interesting enough to warrant a further extension in the study of this problem, with the view of determining how reliable the results which we obtained were, and also to search for an explanation.

Experiment II

Purpose and Procedure. The purpose in this experiment was to continue the same general problem, but to introduce several changes in procedure. This time a record was made of both breathing and pulse.

The pulse record was obtained by adjusting a cardiograph to the neck and placing it so the movement of the carotid artery was recorded. The breathing was recorded by means of a pneuomograph which was placed around the subject at the upper thoracic region. The pneumograph and cardiograph were hooked up so that both wrote on the same smoked drum simultaneously.

The Ranschburg Exposure Apparatus was used for presenting the words. It permits only one word to be visible at one time. A metronome was used to connect the current from dry cells through the apparatus which turned the disk upon which the words were written. The interval between the presentation of the words was two seconds. Another connection was made from the metronome to a signal marker which wrote upon the drum each time a word was shown. A timer was used to mark off intervals of one second so that an accurate count of the breathing and pulse rate per minute could be made.

The kymograph and metronome were located in a room adjacent to that occupied by the subject. The experimenter operated from this room and was never in the room with the subject while the learning was being done.

Before starting the experiment on each occasion the subject was allowed to rest five minutes after the recording apparatus, previously described, had been attached. During the latter part of this rest period records were made of the pulse and breathing rate. These records are considered the norms for the purposes of this experiment, as we were only interested in the relative changes so that these changes might be compared with the changes in learning.

To produce what we shall call an emotional stimulus a .32-caliber pistol was fired in the same room and near the subject. The subject was not allowed to see the gun until after the shot. It was thought that this would be a rather strong emotional stimulus and that it would be reasonably uniform. Perhaps it may be assumed that the physiological changes produced are a rough index to the effectiveness of the shock. After the shot the subjects were always permitted to see the experimenter place the pistol in a table drawer and they were told that there would be no other shot. This was done to avoid the anxiety which might arise due to fear of another shot.

The other stimulus used was designed to be of a non-emotional type and yet produce changes in the pulse and breathing. The sub-

jects were given exercise immediately before presenting the material to be learned. To obtain a fairly uniform amount of exercise each subject was asked to place one foot on a chair, hold on to the back of the chair with the hands, and by jumping to raise the other foot about ten inches from the floor. This act was reproduced twenty-five times with each foot. The words were then presented. The time elapsing between each type of stimulus and the beginning of the learning was approximately thirty seconds.

The subjects were 40 college students, 27 men and 13 women, who were taken from the beginning psychology classes. The material presented to be learned consisted of four lists of one-syllable words.

Each list of words was presented only once. The subjects served on two different occasions. Thus one experimental and one control period of learning was given each time. The control period came first on one visit and the experimental first on the other.

The lists of words were designated by the letters A, B, C, and D. The first subject started the series by learning list A first, and continuing at the other period in order with B, C, and D. The next subject started with list B and learned them in order, getting to list A last. The third subject started with list C and the fourth with list D. The order of presenting the lists was rotated for the purpose of counteracting any difference in the difficulty or position of the words used in the various lists.

The instructions given to the subject were the same in essentials as those in the first experiment. Recall was again used as the measure of learning. The rest periods and reading were given as before. However, this time we used the subjects on two occasions which gave us records of four kinds of periods: normal, which was the rate for the last minute of the five-minute rest period; control, which was the learning period without a preceding stimulus; and two different experimental periods in which learning was preceded either by a shot or by exercise.

The data were tabulated and various comparisons made on the first 30 subjects, and then 10 more subjects were added. The results from the two groupings were so similar that it was decided that the addition of more subjects would not materially change the results or conclusions.

Results. When tabulating the results we made a comparison of

TABLE 2

A Summary of Tables 1 and 2, which Shows the Number of Individuals Who Increased or Decreased in Different Comparisons

When exercise was the stimulus		Number of subjects: Pulse I	Pulse S	Pulse D	Breathing I	Breathing S	Breathing D	Words recalled I	Words recalled S	Words recalled D
Change from normal to control period	Men	21	1	5	22	1	4			
	Women	20	2	1	8	2	3			
	Total	31	3	6	30	3	7			
Change from normal to experimental period	Men	13	4	10	24	0	3			
	Women	9	2	2	10	1	2			
	Total	22	6	12	34	1	5			
Change from control to experimental period	Men	5	2	20	17	4	6	9	5	13
	Women	5	1	7	6	3	4	7	1	5
	Total	10	3	27	23	7	10	17	6	18
When a shot was the stimulus										
Change from normal to control period	Men	24	3	1	18	5	4			
	Women	10	1	1	5	5	3			
	Total	34	4	2	23	10	7			
Change from normal to experimental period	Men	18	1	8	13	7	7			
	Women	10	1	2	6	1	6			
	Total	28	2	10	19	8	13			
Change from control to experimental period	Men	11	2	14	6	4	17	13	5	9
	Women	5	0	8	8	0	5	4	1	8
	Total	16	2	22	14	4	22	17	6	17

I Increase
S Same
D Decrease

the records for individuals on the basis of pulse and breathing changes and the words learned under the different conditions. The results from the various comparisons have been given in summarized form in Table 2. We scarcely expected to get a one-to-one relationship, so we made three divisions: those which increased, those which remained the same, and those in which there was a decrease. The results obtained from women and men subjects were kept separate as it was thought that there might be a sex difference.

The period preceding the stimulus or learning has been called the normal; control refers to the period in which there is learning alone, and the experimental is the learning period following either the shot or the exercise.

Comparison of normal and control periods in regard to pulse and breathing. Since the conditions were the same for the control and normal periods where either exercise or shot was to be used as the stimulus for the experimental period, except that they were taken at different times, these results can be combined. The total for both women and men in the first row of figures in Table 2 shows an increase of pulse in the control over the normal to be 31; 3 were the same, and 6 subjects had a decrease. In the same comparison where the shot was to be used the totals were: increase, 34; no change, 4; and decrease, 2. When these are combined it will be seen that 65 of the 80 cases to be considered reveal an increase in pulse rate for the control over the normal; 7 remained the same, and 8 decreased.

TABLE 3

THE AVERAGE PULSE AND BREATHING RATES PER MINUTE DURING EACH PERIOD RECORDED, AND THE AVERAGE NUMBER OF WORDS LEARNED IN THE CONTROL AND EXPERIMENTAL PERIODS

	Pulse	Breathing	Words
When exercise was the stimulus			
Normal	81.5	17.9	
Control	88	19.2	7.05
Normal	81.1	17.5	
Experimental	84.4	20.6	6.65
When a shot was the stimulus			
Normal	79	17.5	
Control	85.5	18.8	6.60
Normal	79.3	17.2	
Experimental	83.7	18.1	6.32

In Table 3 we see that the two average pulse rates per minute are 81.5 and 79 respectively for the normal periods before the control periods. By combining these we get an average of 80.3. The combined average of the two control periods is 86.8; differences of 6.5 between these averages is reliable, statistically, since it is more than three times the standard error which is only 2.1. Therefore, by either group or individual comparisons the pulse rate during learning is greater than the normal rate.

In comparing the breathing records we find, when we combine the results for the two visits which each subject made, that in 53 of the 80 cases the rate of breathing was more rapid in the control than in the normal period. Thirteen remained the same, and 14 decreased. Taking the group average for the normal rate we find it to be 17.8, while the average for the control is 19. The difference in the averages is 1.2 with a standard error of .51, which is nearly half as great as the difference and therefore not statistically important.

Comparison of the experimental and control periods. For both the stimuli the pulse rate was less in the experimental than in the control periods. When exercise was given 10 subjects had an increase over the control, 3 no change, and 27 a decrease (see Table 2). The decrease was not quite as noticeable following the shot. In this case 16 increased, 2 remained the same, and 22 decreased. These totals are likely to be misleading since there seems to be a sex difference which interferes. Close observation of Table 3 will reveal this difference. Following the exercise 5 of the 13 women had an increase in pulse, which is 38 per cent of them. Only 18 per cent of the men had a similar increase, or 5 of the 27. A somewhat comparable difference is also found in the learning where 54 per cent of the women learned more words in the experimental period, while 33 per cent of the men learned more. Thus a greater percentage of the women increased in both pulse and amount learned when exercise was used than of the men.

Opposite results were found where the shot was the stimulus, for here a greater percentage of men had an increase in their learning than of women. Eleven, or 41 per cent, of the men had an increase in pulse, while 5, or 38 per cent, of the women increased. Forty-five per cent of the men increased their learning in the experimental period, and 31 per cent of the women increased.

The comparison of the group averages presents some evidence that better learning accompanies greater pulse rate. The average pulse rate in the control which accompanied the exercise was 88 (see Table 3), and the average number of words learned was 7.05. The average pulse rate in the experimental was 84.4, and the average number of words learned was 6.65. When the shot was the stimulus the pulse average was 83.7 and the words 6.32, while in the control both pulse and learning were more: the average of the pulse was 85.5, and the words recalled averaged 6.60. Since there were only 40 cases to be considered and the differences are not large, it was found that in each comparison the difference in the averages was less than three times its standard error and consequently the results are not statistically reliable.

The relationship between the breathing rate and amount learned seems to be a negative one, and the two stimuli apparently affect men and women in a different way. For example, more men showed a decrease in breathing rate where the shot was a stimulus and more women an increase. For each sex the tendency in learning was the reverse. More men increased their learning and more women decreased theirs in this experimental period.

Consequently these opposing tendencies of the two sexes under the same experimental conditions has a counteracting effect when we total the results from both sexes together. If we base our judgment upon the results of learning under what we have presumed to be emotional and non-emotional situations, which are shown in Table 3, we would be unable to predict which condition is more likely to be beneficial or a hindrance to learning, unless the sex is known. If only men were used we might expect the learning to be greater following an emotional stimulus. If women were used, the non-emotional stimulus would seem to give better results.

A consideration of the amount learned when the breathing and pulse are considered in relation to the normal rate. Table 4 was made for the purpose of studying the learning when there was an increase, decrease, or no change in the pulse in the control and experimental periods as compared to the normal. There were four learning periods: two control and two experimental. In order to get the totals given in this table it was necessary to compare each individual's pulse rate and breathing in these periods with the normal period immediately preceding it. When this division had been

made the number of words learned in any period was listed under the appropriate heading. That is, if the pulse had been more rapid while the learning was being done than in the normal the number of words learned was listed under the "Increase" column, and so forth. The words were then totaled and the averages which are given in Table 4 determined.

TABLE 4

A Summary Showing the Average Words Recalled in the Three Conditions

	Men	Average words	Women	Average words	Both	Average words	
			Pulse changes and words recalled				
Decrease	27	6.1	4	6.8	31	6.16	S.D. 1.8
No change	6	7.7	9	8.2	15	8	S.D. 2.4
Increase	75	6.2	39	7.3	114	6.62	S.D. 2.1
			Breathing changes and words recalled				
Decrease	19	6.4	14	7.8	33	7.10	S.D. 2.0
No change	12	6.6	9	7.6	21	7	S.D. 2.2
Increase	77	6.1	29	7.4	106	6.49	S.D. 2.0

Under the column where both men and women are combined, and opposite "Decrease," we find the number 31. This is the total number of cases where there was a decrease in pulse rate. The average number of words learned for these 31 cases was 6.16. Below that we see that there were 114 cases where the pulse was increased and the average number of words learned was 6.62. The difference in these averages is .48 (P.E. .26). The chances are about 90 in 100 that the averages are truly different and that there would be more learning with this group under the same conditions when the pulse was increased. The increase in learning is about 8 per cent, which is similar in amount to the increase which Bills (2) and Stroud (17) found in some of their studies upon learning during muscular tension.

For the small number of cases showing no change in pulse rate the learning is greater than for either of the other changes. This may indicate that any stimulus which gives rise to a change in pulse rate is detrimental, but that those causing a decrease are more detrimental. However, it seems more likely that we have

too few cases to be reliable, and that this tendency might change in either direction.

The data for breathing has been treated in the same way in the lower part of the table. In the 32 cases where the breathing rate decreased the average number of words learned was 7.10, and in the 106 cases where there was an increase the average was 6.49. The difference in these averages is .61 (P.E. .28). The chances are, therefore, about 93 in 100 that there is a true difference in the averages, and that more was learned when the breathing rate decreased. This increase in learning was approximately 9 per cent.

For the most part, we seem to avoid the apparent sex difference when we divide our data upon the basis of whether there was an increase, no change, or decrease in pulse or breathing rate, disregarding the stimulus used. The women learned more words on an average than did the men, but the changes in pulse, breathing, and amount learned seem to retain their relationships. If this is true perhaps an emotional or non-emotional situation affects men and women differently and these effects are reflected in the physiological changes, such as breathing and pulse, and in changes of learning in a somewhat corresponding way.

Summary. In this experiment we attempted to determine the relationship between changes in pulse and breathing and changes in the amount learned, after presumed emotional and presumed non-emotional conditions.

Records of the pulse and breathing rates were made with suitable apparatus. Simple one-syllable words were used for the learning material, and the recall of these words was the criterion of how much had been learned. Two types of stimuli which affected pulse and breathing were used immediately preceding the learning, one supposedly emotional in nature (pistol shot) and the other supposedly non-emotional (muscular exercise). Forty subjects were used in the experiment and each acted as a subject on two occasions. This gave two experimental and two control periods for each.

After the emotional stimulus it was found that more men increased than decreased in their learning, but more of the women decreased. The results were reversed following the so-called non-emotional stimulus, and the sex difference continued. More of the men decreased in the amount learned after exercise while more of the women increased in theirs. When the results were combined the

opposing tendencies so balanced each other that there was not a clearly marked common difference in the effects of the two conditions.

There appears to be a positive relationship between increase in pulse rate and increase in the amount learned. When the pulse was accelerated the learning was about 8 per cent more than when it was depressed. This is supported by both individual and group comparisons. The amount learned was greater when the breathing rate was slower. The gain in learning was about 9 per cent when the breathing rate was less. The evidence seems reasonably conclusive that the pulse rate, and probably the breathing rate, is increased over the normal while the subject is engaged in learning of this type, and when there is no extraneous stimulus given.

Experiment III

Purpose and Procedure. The purpose in this experiment was to make a further check upon the results already obtained and to try to use a stimulus which would give a consistent physiological change.

The procedure was in general the same as in the second experiment. There were two variations to be noted: (1) the difference in the stimulus, and (2) taking the pulse rate and breathing for a period of about fifteen seconds after the completion of the stimulus and before the words were presented for learning. In this experiment the breathing was voluntarily increased by each subject. It was thought that this might give rise to a consistent increase in pulse rate as there is a known relationship between the rates of breathing and pulse. In order to find out whether there was an increase in the pulse rate for every individual which was not due in part, at least, to the effort in learning, records were taken after the deep breathing and before the learning began.

The instructions in this experiment were the same as in the others except that the subjects were told to take ten deep breaths. The records revealed that the subjects did breathe more deeply than normally.

Twenty-five subjects were used in this experiment, 15 men and 10 women, taken from the beginning psychology classes. The subjects came only once for this experiment, so we obtained but two learning records: one in the control period, and one in the experimental period.

TABLE 5

A Summary which Shows the Number of Individuals Who Increased, Remained the Same, or Decreased in the Different Comparisons

		Pulse			Breathing			Words		
		I	S	D	I	S	D	I	S	D
Change from normal to control period	Men	12	1	2	12	2	1			
	Women	7	1	2	7	2	1			
	Total	19	2	4	19	4	2			
Change from normal to experimental period	Men	14	0	1	14	2	9			
	Women	9	0	1	4	1	5			
	Total	23	0	2	8	3	14			
Change from control to experimental period	Men	10	2	3	0	2	13	6	4	6
	Women	4	2	4	0	1	9	5	1	3
	Total	14	4	7	0	3	22	11	5	9

I Increase
S Same
D Decrease

Results. In this experiment as in the others we did not get a consistent change for either pulse or breathing for all of the subjects in the experimental period over the control period. During the fifteen-second period after the deep breathing was done the breathing records were too irregular to count accurately and the records from this period have not been included in Table 5. Many of the subjects did not breathe during this time, or breathed too lightly to be recorded. However, the pulse rate was accelerated in 23 of the 25 subjects. The other two were the same. This greater rate before learning did not insure a greater rate during the experimental period than in the control for each subject, as may be seen in Table 5. This table shows that 14 had an increase in the experimental over the control period, 4 were the same, and 7 decreased.

The same general relations hold for the pulse and breathing when compared with the amount learned as in the other experiments. In the experimental period more individuals had an increase in pulse rate than a decrease, and the same is true for the learning. In neither case is the increase very great. As in the others, also, there were more subjects who learned more when the breathing decreased. This continues to indicate a negative relationship between changes in breathing rate and amount learned.

There is little or no evidence of a sex difference shown in this experiment. This is perhaps not surprising, for some of the subjects reported that the excessive breathing made them slightly dizzy, while others were not affected in this way. It seems probable that the experience was of an emotional nature to some of the subjects and not for others. This did not follow the sex line, and judging from our other experiment, we would not expect to obtain a difference in the results from the two sexes.

Summary. By using voluntarily increased depth in breathing we obtained changes in the pulse rate which continued to have a positive relationship with changes in the amount learned. The seemingly negative relationship between amount learned and changes in breathing rate remained about the same. No observable difference in the sexes was found. The average rates for breathing and pulse were greater during the control period when the subjects were learning than in the normal period which was taken shortly before the control record.

General Summary and Conclusions

The problem in these experiments was to study the relationship between changes in learning and changes in pulse and breathing rates when these changes follow supposedly emotional and supposedly non-emotional stimuli. Comparison was also made of the influence of learning upon pulse and breathing rates where no extraneous stimulus was used. Within the limits of these experiments we may draw the following conclusions:

1. There is evidence that the pulse and breathing rates are increased over the normal or resting rates while the subject is attempting to learn. This supports the conclusions arrived at in several earlier studies such as those of Billings and Shepherd (1), and Gillespie (10).
2. Where a stimulus is used to produce an artificial change in pulse rate, if there is an increase in the pulse this is usually accompanied by an increase in the amount learned; a decrease is accompanied by a decrease in learning.
3. An artifically induced increase in breathing is accompanied by a decrease in the amount learned, a decrease by increased learning.
4. There was a tendency for the men to learn more after the emotional stimulus than in the control period and for the women to learn less.
5. After the non-emotional stimulus the tendency was for the women to learn more and the men less.
6. None of the four stimuli used elicited a consistent change in the rate of pulse or breathing for all of the subjects. There would be advantages in using a stimulus which would give consistent changes if this can be found.

If an explanation of the increase in learning is to be based upon nutritional changes the blood flow and amount of oxygen consumed should be measured. However, care must be taken not to interfere with the learning process while recording the physiological changes. If the explanation of more learning lies primarily within the neural connections or increased sensitiveness of the organism rather than in the nourishment of neurones, then speed and strength of the pulse and breathing is perhaps more important than metabolic changes.

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Berea College
Berea, Kentucky

UNE ÉTUDE DE LA RELATION ENTRE LES CHANGEMENTS DE RESPIRATION ET LA VITESSE DU POULS, ET LA QUANTITÉ APPRISE SUIVANT DES STIMULI SUPPOSÉS ÉMOTIFS ET DES STIMULI SUPPOSÉS NON ÉMOTIFS

(Résumé)

Le problème dans ces expériences a été d'étudier la relation entre les changements dans l'apprentissage et les changements dans la vitesse de la respiration et celle du pouls quand ces changements suivent des stimuli qu'on suppose émotifs et ceux qu'on suppose non émotifs. On a fait aussi une comparaison de l'influence de l'apprentissage sur la vitesse du pouls et celle de la respiration où l'on n'a employé aucun stimulus étranger intentionnel. Pour obtenir un soi-disant stimulus émotif on a employé dans l'une expérience une chaise tombante sur laquelle le sujet était assis, et dans l'autre on a tiré avec un pistolet derrière le sujet. On a employé l'exercice et une respiration volontairement augmentée comme stimuli non émotifs. On a fait des notations de la vitesse du pouls et de celle de la respiration dans les diverses périodes et on a comparé ces notations plus tard à l'apprentissage dans chaque période respective. Dans les limites de ces expériences on peut arriver aux conclusions suivantes: Il existe de l'évidence que la vitesse du pouls et celle de la respiration deviennent plus grandes que les vitesses normales, ou cells de repos, pendant que le sujet essaie d'apprendre. Ceci soutient plusieurs études antérieures dans ce champ. Quand on emploie un stimulus pour produire des changements artificiels dans la vitesse du pouls, s'il y a une augmentation de la vitesse du pouls, celle-ci est ordinairement accompagnée d'une augmentation dans la quantité apprise; une décroissance est accompagnée d'une diminution de la quantité apprise. Une augmentation de la respiration artificiellement produite est accompagnée d'une diminution dans la quantité apprise; une décroissance est accompagnée d'une plus grande quantité apprise. Les hommes ont tendu à apprendre plus après le stimulus émotif que dans la période de contrôle et les femmes ont tendu à apprendre moins. Après le stimulus non émotif les femmes ont tendu à apprendre plus et les hommes moins. L'explication de ces relations n'est pas claire. Les changements dans la tension musculaire et les changements accompagnants dans l'attention peuvent donner une explication.

BAKER

EINE UNTERSUCHUNG DES VERHÄLTNISSES ZWISCHEN VERÄNDERUNGEN DES ATMENS UND PULSSCHLAGES UND DIE MENGE DES GELERNTEN MATERIALS, DIE AUF VERMUTETE EMOTIONAL- UND NICHTEMOTIONALREIZE FOLGEN

(Referat)

Das Problem in diesen Untersuchungen war das Studium des Verhältnisses zwischen Veränderungen im Lernen und Veränderungen im Pulsschlag und Atmen, wenn diese Veränderungen auf scheinbare Emotional- und Nichtemotionalreize folgen. Ein Vergleich zwischen dem Einfluss des Lernens auf den Pulsschlag und das Atmen wurde gemacht, wo kein absichtlicher, äusserer Reiz gebraucht wurde. Um den sogenannten emotionalen Reiz zu erhalten, wurde ein fallender Stuhl in dem einen Experi-

ment, auf dem sich die Vp. sich gesetzt hatte, gebraucht, und in dem anderen wurde eine Pistole hinter der Vp. geschossen. Uebung und willkürlich vermehrtes Atmen wurden als nichtemotionale Reize gebraucht. Aufzeichnungen des Pulsschlages und Atmens in den verschiedenen Perioden wurden gemacht, und diese Aufzeichnungen wurden später mit dem Lernen in jeder einzigen Periode verglichen. Innerhalb der Grenzen dieser Untersuchungen können wir die folgenden Schlüsse ziehen Evidenz wurde gefunden, dass der Pulsschlag und das Atmen über die normale oder Rastschnelligkeit beschleunigt werden, während die Vp. zu lernen versucht. Dies bestätigt mehrere frühere Studien auf diesem Gebiet. Wo ein Reiz gebraucht wird, um künstliche Aenderungen des Pulsschlages zu erzeugen, wenn es eine Beschleunigung des Pulsses gibt, wird dies gewöhnlich von einer Zunahme des Lernmaterials begleitet; eine Verlangsamung wird von einer Verminderung des Lernens begleitet. Eine künstlich erzeugte Beschleunigung des Atmens wird von einer Verminderung des gelernten Materials gefolgt; eine Verlangsamung von einer Vermehrung des Lernens. Es bestand die Neigung bei den Männern mehr zu lernen nach dem emotionalen Reiz als während der Kontrollperiode und bei den Frauen weniger zu lernen. Nach dem nichtemotionalen Reiz neigten die Frauen mehr und die Männer weniger zu lernen. Die Erklärung für diese Verhältnisse ist nicht klar. Veränderungen der Muskelspannung und Begleiterscheinungen der Aufmerksamkeit dürften vielleicht eine Erklärung dafür geben.

BAKER

CONSCIOUSNESS VERSUS MECHANISMS IN THE THEORY OF PERCEPTION*[1]

From the German University of Prague

JOHANNES LINDWORSKY

This article is directed against the use of "mechanisms" in psychology and not against the mechanical devices employed in our laboratories. For just as in the development of civilization the discovery and application of tools by man has made for cultural progress, so in experimental psychology the introduction of apparatus permitting exact and controlled measurement of both the stimuli and the reactions of observers has led to far-reaching methodological advances. Our criticism applies rather to physiological "mechanisms" sometimes thought of as "explaining" complicated perceptual operations.

This should not, however, be taken as a criticism of all theories of physiological mechanisms. It will be desirable to show by examples which physiological mechanisms we reject and which we accept as scientifically valid, as well as to point out the characteristics of both sorts.

I. AN EXAMPLE OF PHYSIOLOGICAL MECHANISMS WHICH MUST BE RECOGNIZED IN PSYCHOLOGY

The rod and cone mechanism required in the duplicity theory (von Kries) to account for day and night vision is a familiar example of this. The following characteristics may be pointed out: (*a*) This mechanism is anatomically demonstrable. (*b*) It has merely to account for the peculiarities of day and night vision. It is not to be expected that this mechanism make intelligible complicated perceptual operations which go beyond the ordinary sensory level. (*c*) The ex-

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planation of day and night vision must be physiological; one cannot have a psychological theory which rests purely on the facts of consciousness.

II. Examples of Physiological Mechanisms Which Should Not Be Accepted

1. *Tactual Localization.* If the skin is touched, it is a simple matter to locate the place that has been touched. In order to account for this perceptual ability a special physiological mechanism has been posited, but, in contrast to the mechanisms referred to in Section I, such an apparatus for tactual localization cannot be anatomically demonstrated. There has been ascribed to it further a perceptual capacity which goes far beyond sensory contents such as colors or neutral lights: It was assumed to mediate localization of touch sensations aroused on the surfaces of our bodies. In an unusually painstaking and extensive survey Spearman (6) has pointed out that tactual localization which actually does take place may be far more simply explained on the basis of an assumption presumably supported by experimental evidence; namely, that the tactual sensations in question are connected with images of the corresponding body region. It is by these that we are given knowledge of the stimulated point. In this example we have, therefore, three characteristics of physiological mechanisms which we reject, which are opposed to the characteristics of the mechanisms we have referred to in Section I.

2. *The Problem of the Apparent Constancy of Position of Objects During Eye-Movement.* If a light stimulus is moved across the retina, one usually has the impression of a moving object, as is the case, for example, when a rather small light is moved in a circle against a darker background. But not infrequently objects appear to us as resting even when the light beams emanating from them do move across the retina. This, for example, is the case when we walk through a room with opened eyes. What pronounced movements the retinal image gives can easily be illustrated by a person carrying a photographic camera with opened objective. The inverted image of the room and its furnishings appearing on the ground glass shows lively movements. We experience essentially the same thing when we read the printed pages of a book or magazine. The eye glides over

the lines, the images of the letters and words move over the retina, yet neither our book nor our magazine appears to be moved.

In order to account for this problem of the apparent constancy of position of objects in the case of the moved eye or even in the case of the moved retinal image, it has been assumed that there is an inhibiting mechanism in the central nervous system which so operates that, in spite of the movements in the sense organs and in the cerebral cortex, only resting optical contents are perceived (4). The reader will at once recognize in this example our three characteristics of a poor psychological hypothesis in the theory of perception: (*a*) No physiological mechanism of this sort can be imagined, let alone anatomically demonstrated. (*b*) This mechanism supposedly does more than mediate simple sensory contents; it is intended to explain the process of localization. (*c*) The end which this hypothesis was intended to serve may be much more easily and simply attained by means of a purely psychological hypothesis. For what does the perception of movement mean? Simply this, that a body, *A*, changes its distance from body *B* and from other bodies. There is formed upon the retina as a result the moving retinal image of the entire room, so that in the image the objects in the room remain in relative rest with respect to one another, as actually corresponds to reality. Moreover, we have the experience that it is *we ourselves* who are moving, and it is through this awareness that objectively false impressions are excluded. The complex experience "subject at rest—object moving" is different in many respects from the complex experience "subject moving—object at rest." From both there are various complex ideational experiences which may easily be produced through *konnektive Einstellungen*. This same explanation suffices to explain the fact that the lines and words of the page of a book do not ordinarily appear to move from above to below.

3. *Zöllner's Distortion Images.* If a black strip is moved horizontally behind a vertical slit in a cardboard screen, the strip appears to the observer to be noticeably wider than would be expected from the physical laws governing the path of the light. Schumann had this unusual phenomenon investigated in the psychological laboratory of the University of Frankfort. He concluded that we must assume some apparatus in the brain that causes the stimulus which is temporally longer to appear as a spatial enlargement. In consequence of the movement, the horizontal strip stimulates the ob-

server's eye longer than when the strip is at rest and is seen through the slit. The factual result is the broadening of the strip, and the explanation according to this theory, with which we here take issue, is accomplished through the aid of a completely hypothetical physiological mechanism, which is expected to convert the temporal into the spatial. Again we have our three characteristics: (*a*) the undemonstrable assumption of physiological mechanism, (*b*) the ability of this mechanism to localize, and, as we shall presently see, (*c*) an hypothesis which gives no real insight into the phenomenon and which can be much more simply explained on the basis of conscious experience.

It occurred to the writer that the apparent broadening of this strip might well result from subjective images. Accordingly he requested Simchowitz to perform further experiments on Zöllner's distortion images in the psychological laboratory of the University of Cologne. In order to determine whether real subjective images appear during such observations so as to broaden the strip moving behind the slit, we arranged the following experiment: On the right and left sides of the vertical slit two narrow slits were cut out. The two on the right side of the slit, however, were covered over with white paper pasted over the back side of the screen. The illumination was carefully regulated in such a way that the background visible through the two slits on the left side appeared exactly as bright to the observer as the two covered slits on the right. The observer, who was not acquainted with the procedure, believed, before the beginning of the experiment, that he was looking through five slits in the same way as in an earlier experiment, i.e., through the main slit and two pairs of subsidiary slits to the right and left of it. When, now, the horizontal strip which was attached to a pendulum behind the slit was moved from left to right, it necessarily was seen by the observer as soon as its advancing end reached the first subsidiary slit on the left, likewise when it passed the second slit, and finally when it crossed the vertical main slit. From this point on, however, it could no longer be seen, since it was hidden behind the cardboard of the screen or the paper over the two subsidiary slits on the right. If, therefore, there was aroused in the observer the expectation that after the band had passed the two left and the main slits it would also pass through the two slits at the right, the observer then had an anticipatory schema which could easily be filled

out with the image of a simple black strip. And this is what did occur. All of the observers with the exception of the author and the one conducting the experiment actually saw the black band being moved through the covered slits, that is, the band appeared lengthened in the horizontal direction. This enables us to give a much simpler explanation of this peculiar phenomenon. No fictitious brain mechanism, which can scarcely be imagined, has converted the temporal into the spatial, but rather an imaginal experience of the simplest sort connects itself with perceptual experience (5).

III. Statement of a Working Hypothesis

The three examples we have referred to of physiological mechanisms assumed but scientifically inadmissible bring us to the statement of a working hypothesis of our own. But perhaps the logician will raise a methodological question at this point. Three cases do not give an adequate basis for induction. It must be noted, however, that we are concerned not with the statement of a proposition arrived at through induction, but rather with the formulation of a useful hypothesis. A working hypothesis ordinarily requires no empirical basis; it can be taken from thin air, so to speak, as long as it contains nothing improbable and justifies itself heuristically. Accordingly we may be guided in the statement of our working hypothesis by these three cases we have reported.

The objection to the hypotheses mentioned in these examples lies in the fact that they assume a psychophysical mechanism in no way amenable to physiological analysis and attribute to it the function of providing localization of sensations. In every case this localization is explicable through simple, empirically demonstrable conscious accompaniments. Our working hypothesis therefore is this: The psychophysical process which is determined by sensory excitation conducted to the cerebral cortex provides us with only qualitative data, e.g., the different qualities of color and light, of tone and touch sensations, and the like. It does not furnish an immediate localization of the stimulus. Instead this localization takes place by virtue of the fact that *other* explicit contents are aroused. We may illustrate this by placing three concentric discs over each other, the external and largest ring being colored red, the next one, somewhat smaller, being blue, while the disc in the center is yellow, so that according to our hypothesis only red, blue, and yellow are given by the psychophysical

process. But localization of the central disc is given not by a direct act of localization through a psychophysical process, but through the consciousness of red and blue, for I can never get to the central disc unless I first experience a definite red, then a blue. If the localization of an impression is not given through the impression itself, then that impression also carries no intrinsic numerical property, for number is derived in its simplest form from the different experiences one has with countable objects. A single tree trunk furnishes many definite impressions which it alone can furnish; two trees give quite different impressions, which no other tree trunks would ever arouse. If these experiences are the concrete basis of our concept of number, the numerical property of an impression is accordingly determined through its localization, so that our hypothesis runs as follows: The psychophysical process determined by sensory stimulation provides only sensory qualities, never a localization and never an enumeration of the same.[2]

IV. The Verification of Our Hypothesis

A few examples may show the fruitfulness of our hypothesis.

1. *Single Vision With Two Eyes.* If one stimulates a definite point on the retina of one eye with a colored light, there results an experience of the color of the impinging light stimulus; if one stimulates an equivalent point on the other eye in a similar fashion, one has a similar experience. If, however, one simultaneously stimulates two identical retinal points with the same light, one sees not two lights but only a single one. The paradox of this reduction of effect with double stimulation of this sort was once explained by reference to physiological mechanisms. Hering in particular proposed as a solution to this problem that the nerve fibers passing from identical retinal points into the central organ unite at some point into a single central nerve mechanism. Accordingly, when the two ends of this nerve, which is single except for the peripheral bifurcation, are stimulated in a similar fashion, only one sensation can arise. Helmholtz has raised vigorous objections against this postulation of a mechanism that is not physiologically demonstrable, yet he himself failed to give a satisfactory explanation of single vision with two

[2]That a "space-value of the retina" may be empirically derived, thereby accounting for many phenomena of localization, has been pointed out in the fifth edition of my *Experimental Psychology* (2).

eyes. Single vision upon the stimulation of identical retinal points follows *a priori* from our working hypothesis (1). If the psychophysical process does not provide localization and enumeration, we have the following situation: Let us suppose that one eye is stimulated and the content blue is given. What is given, however, is not mere blueness nor yet "blue here" or "blue there". Now if the other eye is similarly stimulated, you neither have blue given twice nor at two places. If blue is experienced, then blue is experienced and that is all there is to it.

2. *Hering's Investigation of Depth Vision.* It has been Hering's work which has formed the most impressive argument for a conception based on the postulation of physiological mechanism in the case of depth vision. Six parallel vertical lines are stereoscopically presented in two groups of three lines each to the left and right eyes. Since they are spaced equally, they fall on identical retinal points and are seen as three lines in a single plane. Now if one slightly displaces the middle line, moving it a little to the right or left, the image of the displaced line falls on cross-disparate points of the retina and the three lines immediately appear no longer in one plane. The middle line now is seen as either in front or in back of the plane of the other two, according to its displacement. Hering's conception is that the cross-disparate points form a unified sensory mechanism. If this is stimulated the impression of depth results.

The many theoretical and experimental objections which have been brought forward since Hering have been unable to overthrow this conception (2). Doubtless the great and well-deserved authority of Hering has caused us to overlook the later researches. It appears to me, however, that we must look to consciousness for that which a physiological mechanism in itself is utterly unable to bring about. Until it is possible for me to carry these ideas through experimentally, I have tried a rather simple experiment which plays off the hypotheses of mechanism and of consciousness against each other. In the groups of lines above referred to, I varied the middle line of the right-hand group qualitatively and also gradually increased the displacement from its middle position. The task of the mechanism of cross-disparation should now be that of uniting the two middle lines into a single one. The experiment shows, however, that this union is prevented through consciousness of the qualitative difference between the lines. It is not merely that unification of the middle

lines proceeds more slowly and with greater difficulty, but quite frequently the qualitative difference between the two middle lines results in the impression of four parallel vertical lines. And this tendency is stronger the greater the qualitative difference. Consequently we may say in this case that consciousness shows itself stronger in a certain measure than physiological mechanism.[3]

3. *The Problem of Tactual Thresholds.* Why are the two points of an esthesiometer perceived first as one, then as two, when they attain a certain distance of separation, the impression of the observer up to this point having been of a single touch extended, perhaps, in length? Here too Hering sought the solution in the conception of a physiological mechanism. His theory of sensory circles may here be assumed as familiar.[4] However, it by no means agrees with experimental observations, and its modification, the assumption of elementary circles, has been rejected by anatomically-trained physiologists, who have pointed out that no such synthesizing mechanism is anatomically demonstrable. Here again we find the simplest solution in the facts of experience, taking as a starting point the working hypothesis we have set forth above and making a single further assumption which is hardly open to objection. This assumption is that tactual sensations are qualitatively similar at similar points on the skin. The greater the similarity, the more difficult is discrimination. For this reason two tactual stimuli impressed upon the surface of the skin in a similar manner cannot be recognized as two unless the esthesiometer points are placed so far from each other that parts of the skin giving qualitatively different impressions are stimulated together, or unless the two tactual stimuli are applied in a different manner. If the two stimulus points are dissimilar, e.g., the one sharp, the other dull, the spatial threshold becomes smaller; or if one is warm and the other cold, an impression of two-ness appears much sooner. If different images are connected with the two impressions, as in the case of the well-known research by Michotte, we have again a basis for facile discrimination. Successive stimulation by

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[3]The views set forth by N. Ach in the 12th Kongress, 1931, involving a compensation in the case of the apprehending together of similar sensory impressions, tend to obscure the problem even more than the hypothesis of physiological mechanism does.

[4]The theory of *"Empfindungskreise"* was also propounded by E. H. Weber. Cf. Boring, E. G., *A History of Experimental Psychology,* p. 110. (Translator's note.)

two similar stimuli gives different impressions because the second falls in the experiential fringe of the first.

V. Conclusions

Reconsidering perception from the viewpoint of conscious accompaniment may cause many of its facts to appear more simple and natural than physiologically oriented research has thus far permitted. We propose a complete discarding of concepts of brain mechanisms. The only thing which must be thought of as in any way modified and structured in the cerebral cortex is the memory residues of the motor areas. The author has tried in his *"Resonanzanalogie"* to show how these may possibly be conceived (3).

Once we accept this way of viewing the problems we can no longer sympathize with the *Gestalttheorie* of the Wertheimer school, which has been so vigorously propagated in recent years: that a psychophysical process of a configurational character in the cerebral cortex explains the experienced *gestalt*. Quite apart from the improbability of the presuppositions, this theory has the fundamental fault that it cannot explain that which appeared to be the first *desideratum* to Ehrenfels, namely, how in terms of experience the *gestalted* impressions differ from the *ungestalted*. The setting-up of a physiological brain mechanism completely beyond the possibility of experimental investigation can never give us the answer to this question.

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German University of Prague
Prague, Germany

CONSCIENCE ET MÉCHANISMES DANS LA DOCTRINE DE LA PERCEPTION

(Résumé)

Cet article est une protestation contre la supposition injustifiable que les mécanismes physiologiques expliquent les phénomènes psychologiques. Les théories valides des mécanismes physiologiques qui se trouvent à la base des phénomènes psychologiques ont trois traits. (1) Le mécanisme peut être démonstré d'une façon anatomique. (2) On ne compte pas qu'il explique les opérations compliquées de la perception qui dépassent le niveau sensoriel ordinaire. (3) On ne peut pas expliquer le phénomène simplement sur la base de l'expérience consciente. Pour répondre aux objections aux théories injustifiables, qui supposent un mécanisme psychophysique entièrement impossible à analyser physiologiquement et y attribuent la fonction de fournir la localisation de la sensation, quoique l'on puisse expliquer la localisation en termes des accompagnements conscients, on présente cette hypothèse: Le processus psychophysique déterminé par l'excitation sensorielle conduite à l'écorce cérébrale ne nous donne que des données qualitatives, et jamais la localisation et l'énumération de celui-ci. On propose, en conclusion, qu'on écarte tous les conceptes des mécanismes cérébraux, sauf ceux des restes de mémoire des aires motrices. Quand on considère les phénomènes psychologiques en termes de leurs accompagnements conscients plutôt qu'en termes de quelques entités physiologiques hypothétiques, on ne peut pas avoir de la sympathie pour le point de vue de la *Gestaltpsychologie,* qui croit qu'on peut expliquer la Gestalt expérimentée par un processus psychophysique de caractère de configuration lequel a lieu dans le cerveau.

LINDWORSKY

DAS BEWUSSTSEIN GEGEN MECHANISMEN IN DER WAHRNEHMUNGLEHRE

(Referat)

Dieser Augsatz wendet sich gegen die ungerechtfertigte Annahme physiologischer Mechanismen zur Erklärung der psychologischen Erscheinungen. Folgerichtige Lehren der Mechanismen, welche den psychologischen Erscheinungen zugrunde liegen, haben drei Eigenschaften: (1) Die Mechanismen sind anatomisch nachweisbar. (2) Es ist nicht zu erwarten, dass sie komplizierte Wahrnehmungsvorgänge erklären sollten, die über die gewöhnliche Sinneschicht hinausstrecken. (3) Das Phänomen kann nicht bloss auf Grund der bewussten Erfahrung erklärt werden. Um den Einwänden gegen die unhaltbaren Theorien entgegenzukommen, die einen psychophysischen Mechanismus voraussetzen, der ganz jenseits der Möglichkeit einer physiologischen Analyse steht, und die ihm die Funktion der Lokalisierung der Empfindung zuschreiben, obgleich die Lokalisierung durch bewusste Begleiterscheinungen erklärt werden kann, wird diese Hypothese vorgeschlagen: Der Vorgang, der durch die Sinneserregung bestimmt ist, die zu der Hirnrinde geleitet wird, gibt uns nur die qualitativen Tatsachen und diese nie mit deren Lokalisierung und Aufzählung. Zum Schluss wird es vorgeschlagen, dass alle Begriffe der Hirnmechanismen, ausser denjenigen der Gedächtnisreste von Motorflächen, verworfen werden sollten. Wenn wir einmal psychologische Phänomene in bezug auf ihre bewussten Begleiterscheinungen statt ihrer hypothetischen, physiologischen Wesenheiten betrachten, können wir mit dem Standpunkt der Gestaltpsychologie nicht übereinstimmen, die behauptet, dass die erfahrene Gestalt durch einen psychophysischen Vorgang in der Form einer Konfiguration, die im Gehirn stattfindet, erklärt werden kann.

LINDORSKY

PLANT BEHAVIOR*

From the Department of Botany of the University of Illinois

HARRY J. FULLER

Psychology has limited itself almost exclusively to observations on animals, probably as a result of its inception in the study of man's behavior and of the obviousness of animal responses. Unfortunately, psychology has been so engrossed in these animal investigations that it has neglected one extensive field for comparative research—namely, the study of plant responses. The non-botanist is prone to regard plants as relatively insensible organisms because of their lack of locomotion and rapid motor responses. A careful observation of plants, however, shows that in spite of their lack of neural and muscular tissues plants do exhibit many responses analogous to those of animals. Some of the similarities which become evident are: the nature of the effective stimuli, the existence of more or less localized receptive zones, the existence of separate receptive and reactive zones and the transmission of excitation between them, the appearance of fatigue, tetanus and summation of stimulation in many reactions, and finally, certain quantitative relations such as specific presentation times, latent times and reaction times, behavior in accordance with Weber's law, the occurrence of duration-intensity constants, etc.

An examination of the volumes of *Psychological Abstracts* and *Psychological Bulletin* reveals few references to plant behavior. No general reviews of the subject have been found. This paper attempts to supply the lack by summarizing for interested psychologists significant work on plant behavior, which up to the present time has been conducted almost entirely by plant physiologists.

I

Plants are dynamic, sensitive organisms capable of many responses to environmental changes. The stimuli which initiate their behavior are nearly as numerous as those which affect animals and in many

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cases are much the same. Moisture, temperature, light, gravity, acidity and alkalinity, gases, contact, pressure, poisons, anaesthetics, and nutrients are the principal stimuli to which plants respond. The degree of sensitivity of plants to some of these stimuli is much greater than that of many higher animals. For example, the tendril of a wild cucumber responds to the small pressure of .00025 milligrams, a pressure which probably lies below the threshold of stimulation in higher animals. In the case of moisture gradients, plant organs respond to smaller differences than do animals. A young lupine root of 1 millimeter in diameter responds to a difference on opposite sides of .04 per cent of the moisture content of the substratum. The discrimination is very nearly as fine in the case of reactions to temperature differences; a corn root growing in a temperature of 4° or 5° C. above or below its optimum will grow in the direction of the optimum temperature when it is presented.

Inherent in the numerous responses of plants to the above-mentioned stimuli is survival value. Roots react positively to the stimulus of gravitation and grow down into the soil, the natural substratum from which they absorb salts and water. Stems react negatively to the same stimulus; they grow upward above the soil into regions penetrated by light. Roots usually react negatively to bright light and move toward crannies or cracks in the soil, which they penetrate. Stems grow toward the light, which is essential for the food syntheses transpiring in the leaf. Plant organs react positively to optimum, negatively to injurious temperatures in many cases; they react positively to optimum water supply, negatively to a deficient and often also negatively to an excessive supply. Plant organs normally grow in the direction of the oxygen supply; yet pollen grains, falling upon the stigma of a flower, develop a negative aërotropism which drives their tubes into the stylar tissues which they must penetrate to reach the waiting seed primordia in the ovary.

In many of these cases, the direction of response is relative and varies usually with reference to optimum conditions. A plant organ bends normally from cold to warm temperatures, but should the heat become too great the bending may occur in the direction of a lower temperature. The flower stalks of Kenilworth ivy in the early stages of their growth are positively phototropic, but when the seeds have matured the reaction becomes a negative one and the fruits are thus pushed into crevices where the seeds are able to germinate. In the

oat plant, the shoot in weak or moderate light is positively phototropic, in strong light, negatively phototropic. In the case of "compass" plants exposed to sunlight, the leaves are usually borne vertically with the edges of the blade directed in a north-south line, the flat surfaces facing in an east-west position, with the result that when the sun is in its midday position, only the edges of the leaves are turned toward it; the broad surfaces receiving light only in the early and the late hours of the day when the sunlight is less intense. If these compass plants are moved into the shade, they soon lose their peculiar leaf orientation.

II

In general, the reception of external stimuli in higher plants is fairly diffuse and may be compared to the reception of heat and cold in higher animals. Often, however, it is limited to a relatively small number of cells in the organ involved. For example, in the case of the bending of stems toward light, the apical tissues are primarily involved in the reception of the stimulus. In this positive phototropic response seedlings of corn and oats have been the usual subjects of investigation because in the early stages of their growth the young leaves are securely held within a continuous sheath, thus forming a thin cylinder of tissue, lacking appendages and admirably fitted for growth and response studies. Researches upon them have shown that the receptive zone of the sheath is limited to a terminal 2 or 3 mm. of the tip. At least two methods of study have been employed in this work: first, the removal of tips of various lengths from young sheaths with subsequent observations upon the behavior of the stumps after exposure to unilateral illumination; second, the covering of normal, intact tips with lead-foil caps of varying lengths and of close fit so as to form effective screens over different portions of the tip. These methods have resulted in the finding that only the terminal 2 or 3 mm. of the sheaths receive the light stimulus. In the case of decapitation of 2 mm. of the tip, no reaction occurs for several hours; after the lapse of some time, however, the regeneration of a new tip restores photoreceptivity, and a positive bending slowly occurs.

In connection with responses to light the receptive regions of leaves should be described. In the reactions of leaves light appears to be received by the expanded portion (the blade) and the excitation transmitted to the leaf stalk, the bending of which brings about the

movement of the blade. It has been found in many cases that the outer walls of blade epidermal cells are arched upward in dome fashion and that these arches act as miniature lenses, focusing the light rays upon the interior of the cell. The fact that these arched walls act as lenses is undeniable, for photographs (even autochromes!) can be made through them; further, these arched walls actually focus the light on the interior of the leaf, but there is doubt concerning the actual mechanism of their action in reception. These specialized cells, or "ocelli," are widely distributed in leaves.

In the geotropic response of roots a positive reaction, the stimulation of the organ by the gravitational field, is localized in the terminal millimeter or two of the root tip, apparently in the cells of the root cap, a sheath of cells which fits in thimble fashion over the young growing tip. In histological preparations of root sections certain starch grains called statoliths are found in the cells of the root cap. These starch grains are movable in the protoplasm and, since they are of greater density than the plasma itself, they tend to accumulate on the lowest walls of the cells, regardless of the position in which the organ rests. If the root is moved from its normal vertical position and the starch grains move through the protoplasm from the originally lowest side to find rest upon the new lowest side of the cell, an excitation is produced which initiates a curvature that tends to restore the root to its original vertical position, with the consequence that the starch grains once more come to lie upon the original lowest wall of the cell. There is little doubt at present that the statolith apparatus is the receptive mechanism in geotropic response. A recent work has shown that the rate of fall of statoliths in root cells, when a root is removed from its customary position, coincides exactly with the rate of bending of the root. Numerous experiments have demonstrated the localization of georeception in the terminal 1-1.5 mm. of root tips. Charles Darwin reported numerous observations upon the geotropic reactions of roots deprived of their tips or with tips injured by caustics, etc.; in practically all of his experiments any serious injury to the tip resulted in a suspension of geotropic response. After a number of hours, subsequent to the formation of new cells upon the injured tip, geotropic response again became established. Darwin laid the foundations of much work on plant movements and recognized clearly the existence of specialized receptor functions in plant organs. His memoir on *The Power of*

Movement in Plants closes with this sentence: "It is hardly an exaggeration to say that the tip of the radicle [young root] thus endowed [with receptiveness] and having the power of directing the movements of the adjoining parts, acts like the brain of one of the lower animals . . . receiving impressions . . . and directing the several movements." Darwin also showed that, in the case of responses of roots to contact stimuli, receptivity is limited to 1-2 mm. of root tip. One of Darwin's most interesting experiments demonstrated that the vibrations of a tuning fork in close proximity hasten the settling of the grains of starch and shorten the duration of the stimulus necessary to produce a response.

Numerous other cases might be mentioned of definite receptive zones in plants, but mention of one or two more will suffice. In the case of the twining tendrils of cucumber and pumpkin, there are specialized epidermal cells the outer walls of which contain pits, through which the protoplasm in these cells may come in nearly direct contact with external objects pressing against the tendrils. These so-called contact papillae are the structures by which the contact stimulus is received and the excitation induced which results in the movement of the twining organ. In the flowers of the barberry the stamens at a touch on the inner surface of their filaments spring suddenly inwards, with the consequence that another comes to lie on the stigma. The reaction is as rapid as many animal reflexes and depends upon the reception of the contact by more or less differentiated receptor cells on the inner side of the filaments. Contact on other parts of the stamen produces no reaction; only when the pressure falls upon the inner faces of these filaments does the response occur.

III

In practically all of the reactions described above, the receptive zones are separated by varying distances from the reactive zones by which the actual response is effected. For example, in the phototropic and geotropic responses of young seedlings, the effector zone is often several millimeters or even centimeters removed from the receptor zone. In tendrils, the contact stimulus may be received in the basal portion, yet the actual bending occurs in the apical region of the organ. In the so-called sensitive plant, *Mimosa,* the leaflets fold together quickly at a slight touch; the excitation travels up and

down the stem, through 50 cm. or more, often at a rate of 200 cm. per minute, and brings about the progressive folding of leaflets above and below the point of stimulation. Obviously in these cases there exists an efficient mode of transmission of the excitation between the various points of response.

Numerous theories arose as this fact became obvious—Bose's colorful plant-nerve theory, electrical theories, acidity-alkalinity change theories, etc. Only within the past two decades has significant experimental evidence begun to show the precise nature of this transmission. Experimental results from a variety of investigations demonstrate rather conclusively that in the reception of many stimuli, chemical agents (plant hormones or "Wuchsstoffe") are released which diffuse rapidly to the reactive zones. Some of the experiments which have been devised to demonstrate this hormone diffusion are quite unique and are worth a brief description. In the case of the *Mimosa* plant, one experimenter has severed a stem transversely and has then joined the two cut ends by a piece of tubing filled with water, so that the two ends are separated by a water column a few millimeters long. When a leaf below the water gap is stimulated, the excitation is transmitted across the gap and produces a response of the leaves above the gap, indicating the passage of a water soluble substance, released by the stimulus, across the water gap to the upper leaves. Extracts of stem tissue have been made and their effect in producing the typical folding of leaves has been demonstrated. The hormone is inactivated by boiling and is capable of diffusion through collodion membranes. Apparently in *Mimosa* this substance is transmitted through the conducting tubes of the stem, the strands which Bose preferred to call "nerves" in his *Plant Autographs and their Revelations*.

In phototropic and geotropic reactions the action of diffusible substances is scarcely to be doubted. If a root, which grows vertically downward, be turned to a horizontal position, it bends downward after two or three hours. If the tip be cut off as the root is placed in the horizontal position, no bending occurs, since, as mentioned before, the receptivity lies in the tip. Now, if we replace the tip, inserting a thin layer of gelatin between the tip and the stump, bending occurs. In other words, the tip receives the stimulus of gravitation and an excitation is initiated which *passes through the gelatin* and brings about a response in the motor zone, some distance

behind the tip. If we place a root on its side for the length of its presentation time, then remove the tip, the reaction occurs in normal fashion; in this case the excitation has begun to pass to the motor zone *before* the tip has been amputated.

Numerous and elaborate experiments upon phototropic responses have been made during the past decade. The work of Went and his colleagues in Holland has been particularly noteworthy. One of the important results was the discovery that, if the tip of a young oat seedling be removed, then replaced on the stump, with a layer of gelatin between the tip and stump, the seedling bends toward light on being illuminated unilaterally. Here, as in the case of the geotropism experiment described above, the excitation is transmitted through a thin layer of gelatin by a substance or substances capable of diffusion through that gelatin. Went has shown that apparently the "Wuchstoff" is constantly produced by the embryonic tissue at the apex of the stem as the stem grows. This "Stoff" diffuses downward equally on all sides of the stem to the region of cell enlargement, where the cells newly cut off from embryonic cells undergo an increase in size. Went believes that this hormone controls or influences the process of cell enlargement and that upon exposure to unilateral illumination it is partially inhibited or its diffusion delayed by the light, with the result that the cells in the enlargement region on the side *toward* the light enlarge less rapidly than those on the shaded side. The more rapid enlargement on the shaded side causes the axis of the shoot to bend toward the light. Went has further demonstrated the growth-promoting nature of the agent in the following experiment. A number of tips of young oat shoots were cut off and placed on thin slices of gelatin or agar, upon which they were allowed to remain for varying periods of time—60 minutes, 90 minutes, etc. The slice was then cut up into cubes and each cube was placed assymmetrically on a shoot stump of an oat seedling. After a while the stump bent *away* from the side receiving the jelly; that is, growth occurred more rapidly *on* the jelly side. Went found the rapidity of bending was proportional to the number of decapitated tips placed upon the gelatine slice and also to the duration of their rest upon the jelly. This series of experiments indicated not only the presence of a mechanism for the transmission of phototropic excitation, but also the presence of regulators in plants consisting of substances produced by one group of tissues influencing the behavior

of other tissues. One may compare these regulators to the secretions of the ductless glands in higher animals, at least so far as their functions are concerned.

Went has performed numerous experiments to support his contentions: carefully controlled experiments to show that the gelatin brings about no mere contact reaction, microscopic work to show rapid protoplasmic motion in the cells of the shoot apex, as a result of which transmission of the "Stoff" might occur, irradiation investigations to show that light has an inactivating effect upon the "Stoff," etc. By means of very ingenious diffusion experiments Went has shown the molecular weight of the hormone to lie in the vicinity of 376. Although there is still some disagreement among botanists concerning this work, Went's technique is above reproach and his interpretations apparently well founded. The work and its implications are generally accepted at the present time—the transmission of excitation in plants by means of diffusible chemical regulators is no longer seriously to be doubted.

IV

The "motor zones" which I have discussed in preceding paragraphs are motor zones only in the sense that they bring about movements. There are no structures in plants comparable to the muscles of animals. In plants changes of position in most cases are brought about by unequal rates of growth in the various tissues of an organ, processes which are too slow to be directly observable but which may nevertheless effect a movement through considerable distances in a few hours time. In positive phototropism, the bending of an organ toward light is brought about by a more rapid growth on the shaded side than on the illuminated side. In geotropism, stems placed horizontally turn upward as a result of more rapid growth on the lower sides, whereas roots turn downward because of more rapid growth on their upper sides. In climbing vines, twining movements are occasioned by irregular rates of growth which occur in various parts of the organ and which constantly shift position.

In some plant reactions (not many, however) the movement is sufficiently rapid to be followed readily by the naked eye. Occasionally, as in the movements of *Mimosa,* the response is fully as swift as an animal reflex. Likewise, in the case of the barberry stamens, mentioned elsewhere in this paper, the movement is as

quick as though it were brought about by a spring release. In the fungus, *Pilobolus,* slender threads rise above the substratum and produce spore cases at their apices; when the threads are illuminated unilaterally, they bend slowly within several minutes toward the light and suddenly—ping!—the spore cases are shot from their stalks at the light with considerable speed, often travelling a distance of as much as five feet.

In these cases of sudden, rapid movements, the response obviously is not a result of unequal growth in different portions of an organ, a relatively slow process. They are rather attributable to sudden changes in turgor, alterations in the water content of tissues, producing changes in rigidity from which movements develop. In the leaves of *Mimosa* there are at the bases of the leaflets and of the leaf stalks cushions of tissue made up of spongy, thin-walled cells containing considerable stores of water. When the plant is presented with a sudden temperature change or a contact-stimulus, these water-storage cells quickly eject their water into the spaces among them, the rigidity of the cushions collapses, the leaflets fold together and the whole leaf drops suddenly as its support is removed. After 10 or 15 minutes, the cushion cells resorb the water from the intercellular spaces, the tissues become increasingly rigid, the leaflets open, and the leaf stalk slowly rises. In the barberry stamens, a similar situation obtains. Here the water-storage tissue lies on the inner faces and bases of the filaments. Upon stimulation by contact, the cells of this tissue lose water and the stamens instantly drop forward to lean over the stigma.

In *Pilobolus* the spore case surmounts a water reservoir containing a concentrated solution of sugars and other organic materials. Through some as yet imperfectly understood reactions within this reservoir, light causes a sudden increase in the pressure and the explosion occurs which catapults the tiny spore case at the light.

Explosions due to increased internal pressure also cause the rupture of many fruits and the consequent scattering of seeds. Such explosions occur in the fruits of the squirting cucumber, which shoots some of its pulp and seeds out through a pore when the internal pressure becomes sufficiently great. The touch-me-not plant scatters its seeds when the seed capsule explodes as a result of unequal tensions set up by pressure differences in the walls of the fruit.

V

After a plant organ responds to a stimulus, the organ usually regains its original condition and becomes susceptible to further stimulation. If several stimuli are presented successively, each one before complete recovery has occurred, the responses become less marked. This fatigue, as it may be called, is a common phenomenon in plant behavior. For example, in the *Mimosa* plant, the leaflets fold and the leaves drop upon contact stimulation. When the plant is thus stimulated at intervals in such rapid succession that complete recovery is not possible between stimuli, the leaves return to their original erect, open position and are no longer responsive to mechanical excitation. If the stimulation is then stopped, the leaves after a definite period of rest become responsive once more. In *Dionaea,* an insectivorous plant, the halves of a leaf blade fold together longitudinally when one of the contact-receiving hairs on the leaf margins is touched. If such a leaf be prevented forcibly from closing and one of the hairs be repeatedly touched, that particular receptor becomes fatigued. If the leaf then be relieved of interference and the over-stimulated hair be touched, no reaction occurs—the hair, we may say, has become exhausted. Unfortunately, fatigue in plants is a little understood subject and must await further study for a satisfactory explanation.

Related to the phenomenon of fatigue is that of summation of stimuli. If a stimulus is presented which lies below the limen for that reaction, no reaction occurs; if this subliminal quantity of stimulation be repeated at the proper intervals, the threshold value of the stimulus is reached and the response is initiated. For example, in oat seedlings if the presentation time for a certain light intensity is 15 seconds, an exposure of 5 seconds produces no response. But a subsequent exposure, within a few seconds, of two or more 5-second periods or one 10-second period is sufficient to reach the threshold and response occurs. A comparable condition occurs in geotropic reactions. In other responses, such as those of *Mimosa,* summation of stimuli is not apparent. In rapid turgor reactions of this type, the threshold stimulus must be presented at one instant, for the reaction is like that produced by the trigger of a gun. A certain minimal pressure is required to snap the trigger and no addition of repeated lesser pressures will ever bring about the response.

VI

In connection with the quantitative aspects of plant behavior (which incidentally have been badly neglected), the question of presentation, latent and reaction times is an important one. As in the case of many animal responses, a given stimulus to be effective must act upon plants for a certain minimal period of time. This is known as the presentation time. In the turgor movements of *Mimosa* leaves, barberry stamens, etc., this presentation time is extremely short. In such reactions as tropisms, however, in which the response is caused by unequal rates of growth in different portions of an organ, the presentation time may be measured in seconds or minutes. Under constant external conditions (temperature, humidity, etc.) and constant internal conditions (age and vigor), the presentation time of a given reaction is found to be approximately constant for the organisms of one species or variety at a given stimulus intensity. In the case of phototropism in the oat seedlings mentioned above, the presentation time for a particular intensity at the receptor was 15 seconds; under constancy of conditions all plants of the same age exposed to that intensity will have about the same presentation time, with some differences due to individual variation.

In phototropism, geotropism, and several other plant reactions, a general law obtains to the effect that the presentation time varies inversely with the intensity. In our oat seedlings if we double the light intensity which requires a presentation time of 15 seconds, in the increased light the presentation time will be approximately 7.5 seconds. That the same relation holds for geotropic response can be demonstrated by increasing the strength of the stimulus through the use of centrifuges operating at controlled speeds.

The latent period and reaction time possess determinable values, as does presentation time, which vary with conditions of temperature, age, vigor, etc. They can be measured and they remain approximately constant in individuals of the same species maintained under identical internal and external conditions.

In connection with this discussion it is of interest to note the short presentation times necessary to induce certain responses and also the short reaction time of many plant responses. The shortest presentation time on record for a geotropic response is 3 minutes (young sunflower stems). The geotropic reaction time of this plant

is in the vicinity of 35-40 minutes at average temperatures. Temperature is one of the important external conditions affecting tropic responses. In the geotropism of seedlings of Windsor bean, for example, the presentation and reaction times at 14°C. are 70 and 120 minutes respectively; at 30°C. they are 10 and 48 minutes respectively. In phototropism the short duration of these periods is even more striking. The broad bean, for example, responds to a single candle at a distance of 22 meters, one of the cresses at 55 meters. In oats, the most sensitive plant known with respect to phototropism, the presentation time is 4 seconds at 5.4 meter-candles; at 1900 meter-candles, the presentation time is .01 second. In these cases of phototropism the intensity-duration product is approximately a constant.

One feature of plant behavior likely to be of interest to the psychologist is found in the correspondence of many plant reactions with Weber's law, which states that when an organism is adjusted to a certain magnitude of stimulus, the least quantity of additional stimulus required to bring about a response always bears the same proportion to the whole stimulus. For example, if certain motile bacteria are grown in a meat extract solution and if an additional concentration of meat extract is admitted to the solution by means of a capillary tube, the magnitude of added extract-concentration required to produce a chemotaxy of the bacteria toward the tube can be determined. If the concentration of the meat in the original liquid is .001 per cent and if a concentration of .003% in the tube is required to cause a positive chemotaxy, then according to Weber's law, within certain limits, this same proportion of added stimulus to original stimulus (in this case 3:1) necessary to cause the response will be the same in all cases. That is, if the concentration of the meat extract were 1%, then a tube concentration of 3% would be the minimum quantity of additional stimulus; if the original concentration were 1.5%, the added concentration would have to be about 4.5%, etc.

Weber's law apparently obtains in a wide variety of plant responses—chemotaxy of bacteria, swimming spores, sperms of ferns and mosses, chemotropism of pollen tubes and fungus hyphae, geotropism in higher plants, thigmotropism, in some cases, at least, in phototropism, etc. The case of phototropism is particularly interesting. It has been known for some time that plants grown in the dark are more responsive to light than plants grown in the light, so far as

phototropic bending is involved. In the case of the sporangial stalks of fungi and apparently in the young shoots of some higher plants, this phenomenon can be explained in terms of Weber's law; namely, that in the case of plants grown in the light for phototropic study the intensity of light used as the phototropic stimulus must be greater, in a definite degree than that of the light in which the plants have been grown.

VII

The cases of plant behavior which are perhaps best known to the non-botanical reader are anaesthesia and sleep-movements. Whatever seductiveness these subjects hold lies not so much in their theoretical significance as in the associations with animal responses aroused by the terms "anaesthesia" and "sleep." In the case of anaesthesia there are certain points of similarity in plant and animal behavior, but the "sleep" of plants is by no means comparable with the relaxed position of animals in sleep. Further, the sleep of plants does not result in recovery from fatigue as it does in animals.

Numerous anaesthetics produce a state of insensitivity to stimuli in plants. Nitrous oxide, for example, causes the reactive stamens of the barberry and the leaves of *Mimosa* to lose their responsiveness. Likewise ether and chloroform produce a cessation of response in these plants, in the insectivorous leaves of *Dionaea,* in the phototropic and geotropic responses of seedlings and in numerous other plant responses. These anaesthetics depress the metabolic rate, affect protoplasmic permeability and result in general insensibility. Upon the removal of the anaesthetic, recovery of responsiveness occurs, if the exposure has not been too greatly prolonged nor the concentration of anaesthetic too great.

The sleep movements of plants are known also as nyctitropic movements. They are produced chiefly by turgor changes in water storage tissues, located at the bases of leaflets, under the periodic stimulation of alternating day and night. In most cases, as in *Mimosa,* clover, sheep sorrel (*Oxalis*), locust, beans, etc., the response consists in a drooping or folding of the leaflets. In some cases, as in the sleep movements of the flowers of *Oxalis,* water-lilies, etc., the reactions result principally from growth differentials rather than from turgor changes. The stimulus which produces the response is the decrease of light intensity which occurs in the late afternoon. The leaves

maintain the so-called sleep position during the night and recover their original position in the early hours of morning when the light intensity increases.

These periodic reactions to regular diurnal variation in light intensity have in many cases become habitual, and definite rhythms may be instituted which often persist under changed conditions. For example, if certain species of *Oxalis* are grown in a greenhouse under normal conditions of illumination during the day and darkness at night, and then if the conditions are reversed—that is, if they are kept in darkness during the day and illuminated at night—they will behave for several days as before, remaining "awake" during the day, even though in darkness, and going to "sleep" at night, even in bright light. In three or four days they become adjusted to the changed conditions and remain open under illumination when it is presented at night, and close in the darkness, though it occurs during the day. If normal conditions are again restored, several days will be required to break the plants of their newly acquired habit and send them back to their accustomed mode of life. With this remarkable exhibition of "memory" in plants, an entirely new set of possibilities is opened up for discussion—and an adequate end provided for this paper.

It has been reported recently by Kögl and Haagen-Smit (Amsterdam K. Akademie van Wetenschappen 34: 1931) that the growth-substances from bread mold, yeast, and grass coleoptiles are probably identical and that they are closely related to a growth substance isolated and purified from human urine. This hormone has been termed "auxin" and is apparently a monobasic acid of the aliphatic series with the formula $C_{18}H_{32}O_5$.

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University of Illinois
Urbana, Illinois

LE COMPORTEMENT DES PLANTES

(Résumé)

Les plantes répondent à beaucoup des mêmes stimuli que les animaux—changements de température, humidité, lumière, gravité, acidité et alcalinité, gaz, contact, poisons, nourritures et anaesthésiques. En quelques cas elles peuvent réagir à des intensités du stimulus au-dessous le seuil pour les réponses des animaux. Ces réponses possèdent ordinairement une valeur survivante et peuvent être positives ou négatives selon la nature et l'intensité du stimulus. La réception des stimuli chez les plantes est généralement diffuse, mais en quelques cas il se trouve des zones perceptives local-

isées d'une manière définie, souvent à quelque distance des zones réactives. La transmission de l'excitation doit exister entre ces zones; on croit généralement que les hormones sont responsables de cette conduction. Dans la plupart des réponses des plantes le temps de réaction ne peut être mesuré qu'en minutes ou heures, mais en quelques cas les réponses ressemblent aux réflexes des animaux dans leur rapidité. Le premier type de réponse est ordinairement le résultat des altérations de la vitesse de croissance, le dernier le résultat des changements subits de la quantité de l'eau dans les tissus. Dans de nombreuses réponses des plantes on peut démontrer la fatigue et la sommation des stimuli. Bien des réactions des plantes ressemblent à celles des animaux dans les aspects quantitatifs: présentation définie, temps de latence et de réaction, lois des constantes de durée-intensité, loi de Weber, etc. Les réponses périodiques ont donné comme résultat chez quelques plantes des rythmes qui sont très difficiles à briser.

FULLER

DAS PFLANZENVERHALTEN

(Referat)

Pflanzen reagieren auf viele Reize wie Tiere—Veränderungen der Temperatur, Feuchtigkeit, des Lichtes, Gewichtes, der Säure und alkalischen Beschaffenheit, der Gase, Berührung, Gifte, Nährstoffe und Anästhesien. In einigen Fällen vermögen sie auf verschiedene Stärken des Reizes unter der Schwelle für Tierreaktionen zu reagieren. Diese Reaktionen besitzen gewöhnlich einen Ueberlebenswert und können positiv oder negativ sein, was von der Natur und Stärke des Reizes abhängt. Die Aufnahme der Reize in Pflanzen ist allgemein verbreitet, aber in einigen Fällen kommen entschieden lokalisierte Wahrnehmungszonen vor, oft etwas weit von den Reaktionszonen. Uebertragung der Reizung muss zwischen diesen Zonen stattfinden; es wird allgemein angenommen, dass die Hormone diese Leitung ausführen. In den meisten Pflanzen können die Reaktionszeit nur in Minuten oder Stunden gemessen werden, aber in einigen Fällen ähneln die Reaktionen den Tierreflexen in ihrer Schnelligkeit. Die erstere Art der Reaktion erfolgt gewöhnlich durch Veränderungen in der Geschwindigkeit des Wachstums, die letztere durch plötzliche Veränderungen in dem Wassergehalt der Gewebe. In zahlreichen Pflanzenreaktionen können Erschöpfung und Summierung der Reaktionen nachgewiesen werden. Viele Pflanzenreaktionen ähneln denen der Tiere in quantitativer Hinsicht: bestimmte Darbietung, latente und Reaktionszeiten, Gesetze der Dauer-Stärke-Konstanten (duration-intensity constants), Webers Gesetz, usw. Periodische Reaktionen haben in einigen Pflanzen Rhythmen zur Folge, welche mit Schwierigkeit gebrochen werden.

FULLER

A RATIONAL EQUATION OF THE LEARNING CURVE BASED ON THORNDIKE'S LAW OF EFFECT*

From The Mooseheart Laboratory for Child Research, Mooseheart, Illinois

HAROLD GULLIKSEN[1]

The purpose of this paper is to give a mathematical statement and development of certain fundamental propositions contained in one type of learning theory, to show that such mathematical treatment makes it possible to determine accurately the agreement between scientific theory and experimental data, and to analyze the experimental material accurately in other respects. Certain parts of Thorndike's (37) Law of Effect will be stated in the form of a group of mathematical functions, and it will be shown that this statement is a generalized form of the learning theory previously developed by Thurstone (40, 41). From these functions one of the equations of the learning curve that is implicit in them will be derived and applied to learning data. Various methods of using this equation in the solution of psychological problems will then be suggested.

Thorndike (37) (1932) assumes that the "stamping-out" effect of each repetition and punishment of the wrong response is *constant* during the course of the learning process. Correspondingly, he assumes that the "stamping-in" effect of each repetition and reward of the correct response is *constant* during the course of the learning process. Thurstone (40) made these same assumptions earlier in his 1930 discussion of the learning function with the additional proviso that they held only for a single subject in a single task. Many other theories (see Hobhouse, Carr, Smith and Guthrie, Holmes,

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Peterson, etc.) assume that there is some sort of strengthening or "stamping-in" of the correct response and a weakening or "stamping-out" of the incorrect response, but there is no precise quantitative assumption regarding the amount of this effect. This effect may remain constant during the learning of a given task, or it may gradually increase, may gradually decrease, or otherwise vary during the course of learning. Which of these possibilities is in agreement with the facts in a given case? This assumption of constancy furnishes a convenient starting-point for mathematical investigation of learning theories.

The possibility is also clearly recognized that another mathematical formulation based on a quite different set of assumptions may lead to a learning function which represents the data more adequately than the present equation. It is also possible that a learning theory of the same general form as that presented here is adequate to cover certain types of learning, but that a quite different theoretical formulation will be found necessary for other types of learning phenomena. Regardless of the final adequacy or inadequacy of the particular postulates used here, the general method of mathematical treatment, of which one form is illustrated here, furnishes a valuable means of analyzing learning data.

The method of stating theories as mathematical functions, deriving certain observation equations from these initial postulates, and checking these equations against experimental data has been called by Troland (43) the method of "mathematical hypothesis." The work of Troland (43) and Hecht (13) on the visual receptor process is one illustration of the application of this method to some problems related to psychology. Mathematical hypotheses have been used extensively in the physical sciences. This method is valuable because it makes possible an accurate quantitative test of the agreement between scientific theory and experimental data.

The use of "mathematical hypotheses" does not mean simply fitting an equation to some data. It means the use of rational as opposed to empirical equations. There seems to be some confusion in the literature regarding this distinction. For instance, Max Meyer's (25) equation, which in essence is an empirical equation, is sometimes referred to as a rational formulation. Barlow (1), instead of recognizing the empirical nature of both Meyer's (25) equation and of Thurstone's (38, 39) earlier equations, states that per-

haps the distinction between an empirical and a rational equation may be the extent to which the parameters can be determined by rigorous methods. It is, therefore, essential to distinguish accurately between rational and empirical equations. Empirical equations are simply equations which happen to fit certain data. They are not derived from scientific hypotheses, and consequently our knowledge of scientific theory is not affected by the use of empirical equations. A rational equation is developed from certain basic theories and represents accurately the relationships that will be found in the experimental data, if these theories are true. If the equation does not fit the data, it proves that the theories in question cannot be applied to the particular data. If the equation does fit, it proves that the theories can be applied to those data. That a given theory *must* be applied to certain data cannot be proved. A discussion of the value of rational equations in advancing the knowledge of scientific theory has recently been given by Gray (12). Rational equations are powerful tools for the advancement of science, because they make it possible to test accurately the agreement between experimental data and scientific theory.

In comparison with the vast quantity of work that has been done on learning, relatively few attempts have been made to give a rational mathematical formulation of the problem. On the whole, there has been no clearly defined connection between the different psychological theories of learning (see Thorndike, Carr, Hull, Tolman, Peterson, Köhler, Watson, Smith and Guthrie, etc.) and the various equations of the learning curve that have been suggested (see review of literature to follow). Perhaps the method has not been used to a marked degree because it is thought that there are certain objections to applying the method of mathematical hypothesis to the problems of learning. Some of these supposed objections will be discussed later in this paper.

In the following survey of the previous literature, only work on the equation of the learning process will be considered, i.e., equations where attainment is considered as a function of practice. Work on equations of the forgetting curve will not be considered; also work will be omitted which deals with the relationship between different attributes of the total learning process, such as the relation between number of syllables to be learned and total learning time. Although about twenty equations of the learning curve have been

suggested by different writers, most of these can be shown to be special cases of six or seven different functions.

One form of equation which has been advocated by different writers from 1907 to 1928 is of the general form

$$y = a + b\ e^{-cx} \quad \text{(referred to as type A)}.$$

This equation is based essentially on the assumption that the rate of learning is proportional to the material still to be learned, or to the difference between the present attainment and the physiological limit of attainment. This assumption may be stated in the form of the differential equation

$$dy/dx = c\ (a - y) \quad \text{(derivative of type A)},$$

in which y is a measure of attainment, x is a measure of practice, a is the limit of attainment as practice increases without limit. Integrating this gives

$$\log\ (a - y) = -cx + \log\ (-b)$$
$$\text{or} \quad \log\ (a - y) / -b = -cx$$
$$\text{or} \quad a - y = -b\ e^{-cx}$$
$$\text{or} \quad y = a + b\ e^{-cx}$$

This initial assumption and the resultant equation have appeared in the literature in several different forms. The special case where $b = -a$ is known in economics as the Mitscherlich-Spillman curve of diminishing returns (26, 36).

In 1907 Schukarew (32) developed this equation, using the analogy of a monomolecular chemical reaction. He started with the differential equation $-dC/dt = kC$; integrating gives

$$\frac{\log C_0 - \log C}{t - t_0} = k$$

where t is time, C is the ratio of the material still to be learned to the total material. At first glance this does not seem to be identical with the form given above. C, however, does not represent attainment, but the amount still to be learned. The total amount to be learned thus is C_0 or the value that C has at the beginning of the experiment. Then the attainment or $y = C_0 - C$, and $dy/dt = -dC/dt$. Making these substitutions, the differential equation assumed by Schukarew, $-dC/dt = kC$, becomes $dy/dt = k\ (C_0 - y)$, which is the derivative of type A given above. We see, then, that Schukarew is making the assumption that the rate

of learning is proportional to the amount still to be learned, only instead of designating the amount still to be learned by $C_0—y$, he designates it as C.

In 1911, Charles Henry (15) used the same equation, writing it as follows: $y=Y\ (1—e^{—\beta x})$ (type A above). He regarded this equation as an empirical approximation to the derivative of the equation $y=Y\ (1—e^{—\beta x})^2$ which he had developed and used in the field of sensation. Henry fitted this equation of the learning curve to various types of learning data, using different sensory and motor habits.

In 1925, Spielrein (35) developed an equation of the learning curve from the assumption that the increase in attainment is "proportionnel à la différence entre la valeur-limite du développement possible pour l'individu et la valeur du développement actuel de l'aptitude en question." He began with the differential equation

$$dy/dx = a\ (y_\infty—y),$$

which on integration gives

$$y = y_\infty—(y_\infty—y_0)e^{—ax},$$

which can be readily seen to be of type A.

In 1926, Ettlinger (6, 7) developed an equation of type A by making the assumption that "the relative rate of decrease of the margin of attainment with respect to the amount of practice is constant." If x=practice, and y=the margin of attainment (i.e., Y=attainment, and L=the physiological limit of attainment, then the margin of attainment (y) as defined by Ettlinger is given by the formula $y=L—Y$), then Ettlinger's assumption may be expressed mathematically by the equation

$(1/y)\ (dy/dx) = —k$; integrating gives
$\log y = —kx+c$
(when $x=0$, $Y=0$ and $y=L$, so $c=\log L$)
$\log y—\log L = —kx$, or
$y = L\ e^{—kx}$

Replacing y by its equivalent $L—Y$ gives

$$Y = L—L\ e^{—kx} \text{ (type A above).}$$

Ettlinger's equation, $y=L\ e^{—kx}$ is in the same form as the equa-

tion developed in 1907 by Schukarew and can readily be seen to be a modification of type A.

In 1928, Heinis (14) gave a function of type A with a slightly different notation. Heinis' equation as given by Gariayeva (11) is

$$y = a - b\ e^{-x/d}$$

Heinis' equation is taken from Gariayeva because Heinis' manuscript is not available.

Husband (16) in 1931 suggested that plotting learning data on semi-logarithmic paper "gives us clearly objective and concise ways of interpretating data." The plot on semi-logarithmic paper is simply the rectified form of the equation

$$y = A\ e^{-bx},$$

which was suggested in 1907 by Schukarew and again in 1926 by Ettlinger. No reference is made by Husband to this earlier work.

We will now consider a second type of learning function. Several writers have suggested equations of the learning curve based essentially on the assumption that the rate of learning with respect to the amount of practice is proportional to the product of the amount already learned and the amount still to be learned. The amount still to be learned is the difference between present attainment and the physiological limit of attainment; or sometimes the difference between the total amount of material to be learned and the amount already learned. Stating this assumption in the form of a differential equation we have

$$dy/dx = a\ y\ (b - y) \quad \text{(derivative form of type B)},$$

in which y is some measure of attainment, x a measure of practice, b is the limit of attainment, and a is a constant probably dependent on the individual learner and the task to be learned. Integrating this equation gives

$$y = \frac{b\ e^{Ax}}{c + e^{Ax}} \qquad \text{(type B)},$$

in which $A = ab$ and c represents the constant of integration. This equation was suggested in 1920 by Pearl and Reed (28) to represent population growth.

Robertson (29) in 1908 developed this equation using the analogy of a monomolecular autocatalytic reaction. He brought forward a body of evidence to show that the process which takes place upon sen-

sory stimulation may be regarded as a monomolecular autocatalytic reaction, and wrote the fundamental differential equation of such a reaction as

$$dx/dt = kx\,(A—x).$$

This equation expressed his fundamental assumption, which was "la vitesse de la transformation est, à chaque instant, proportionnelle à la quantité $(A—x)$ de matière qui subit le changement et à la quantité de matière x qui a déjà été transformée." Integrating the above equation, Robertson gets

$$\log \frac{x}{A—x} = K\,(t—t_0)$$

taking anti-logarithims and solving for x gives $x = \dfrac{Ae^{Kt}}{e^{Kt_0}+e^{Kt}}$, which can readily be seen to be of type B. Robertson finds that certain data taken from Ebbinghaus were fitted by another equation derived from this primary one. He also mentions the learning curves of Bryan and Harter and of Swift, regretting that their data are given graphically and not numerically so that he cannot determine accurately whether or not his equation fits their data. In 1915 (30) and again in 1923 (31) Robertson published tables and directions for computing this curve.

Culler (5) in 1928 put forward the view that the complete learning curve always has an inflection point and suggests either the monomolecular autocatalytic equation or the phi gamma curve to fit learning data.

Chaisson (3) in 1930 regrets the little attention attracted by Robertson's work and uses the monomolecular autocatalytic equation to fit certain learning data.

Valentine (46) in 1931 again developed this same equation, stating as his basic assumption that "the rate of improvement in any function with respect to the amount of practice is proportional to the amount already learned up to any specific practice level and the amount yet to be learned." He gives as his fundamental differential equation

$$dy/dx = ky\,(1—y)$$

and on integrating this arrives at the function

$$y = \frac{le^{klx}}{c+e^{klx}}.$$

It can readily be seen that, save for notation, this is identical with the equation of a monomolecular autocatalytic reaction suggested in 1908 by Robertson. Valentine in this same paper also developed a slightly more generalized form of this function "to account for the effects of whatever inherited sensorimotor substratum and modification of it bear on the specific function under examination." This substratum is represented by the parameter "a" and the fundamental equation changed to

$$dy/dx = k\,(y—a)\,(l—y),$$

which on integrating gives

$$y = \frac{le^{k(l—a)x}+ca}{c+e^{k(l—a)x}}.$$

A group of equations of the learning curve will now be considered which were originally selected because the equations more or less accurately fit certain learning data.

In 1914 Foucault (8) published some data on improvement with practice using addition problems. This was fitted by the hyperbola

$$e = a + \frac{ab + c}{T — b},$$

in which e represents the "economie" or the difference between the time taken to add the first column, and the time taken to add the n-th column; and T represents the total time which the subject has been working. The use of the hyperbola to fit the data was apparently due to the mathematician, M. Fabry, to whom Foucault presented his data for mathematical representation. In later publications, 1920 (9) and 1926 (10), Foucault again used the hyperbola as a mathematical expression of "the law of exercise."

Thurstone (38) in 1917 also suggested using a hyperbola to fit learning data. He examined some forty different empirical equations and concluded that the hyperbola

$$y = \frac{a(x + c)}{(x + c) + b}$$

was the best one to use.

In this equation x represents total number of practice acts, y is attainment in terms of number of successful acts per unit of time, "c" represents previous practice, "a" represents the physiological limit,

and "*b*" is a number representing the rate of learning. In 1919 Thurstone (39) published a monograph giving a more complete account of his work on this equation of the learning curve.

In 1923 Meyer (25) pointed out the strictly empirical nature of Thurstone's equations and attempted to develop a more rational equation of learning. However, his arc-cotangent function was not derived rigorously from basic assumptions regarding learning, but was selected arbitrarily from among a number of functions which had the same general mathematical properties, namely, an asymptote and an inflection point.

In 1927 Leiner (19) attempted to develop an equation of learning in which both practice and fatigue effects would be accounted for. He bases his equation on a number of apparently arbitrary assumptions, one of them being that a pure practice-increase first occurs in the fourth three-minute interval of practice.

Jette (17) in 1928 worked on the learning curve for two different forms of shorthand. He found that an empirical formula,

$$y = x/(a + bx),$$

taken from Lipka and resembling Thurstone's hyperbola, gave a close fit to the data for learning the Gregg shorthand. This equation, however, did not fit data for learning another type of shorthand, but Jette found that an equation given by Einstein in 1907 to describe the variation in the atomic heat of a solid with temperature was rather good for fitting these latter data.

In 1929 Courtis (4) applied to learning data the curve developed by Gompertz in 1825 to describe mortality statistics. The function is

$$y = k\ g^{c^x},$$

in which y represents growth, x represents time, k is the maximum value, g the initial value, and c represents the rate of development.

Moore (27) in 1932, by a strictly empirical method of selecting the function which gave a good fit to his data, arrived at the same curve, writing it in the logarithmic form

$$\log y = a + bc^x.$$

Writing Courtis' equation in the logarithmic form gives

$$\log y = \log k + (\log g)\ c^x,$$

which except for notation can be seen to be identical with Moore's equation. As Moore interprets his equation, the constant a equals the logarithm of the maximum value or the physiological limit, and c represents the rate of development. The smaller c is, the more rapid the rate of development. Moore's interpretation of the constant *"b"* is, however, slightly different from the interpretation of the corresponding constant as given by Courtis. To explain *"b,"* Moore introduces another constant *"d"* which is equal to $b(1—c)$. This value *"d"* is "a function of the individual's fundamental equipment prior to learning."

In 1930 Kern and Lindow (18) put forward a logarithmic equation of the learning curve,

$$\dot{U} = (1/k) \log Z + C,$$

in which $\dot{U}$ represents the *Übungswirkung,* and Z the *Übungszeit.* After finding empirically that the logarithmic function gave a good fit to a variety of data, a "rational" development of this equation is given, based, so the authors state, on the assumptions that the inhibition-factor (*Hemmungsfaktor*) and practice-effect (*Übungswirkung*) are each directly proportional to the practice-time (*Übungszeit*) and inversely proportional to each other. It is, of course, impossible for two variables each to be directly proportional to a third variable and inversely proportional to each other. Stating the assumptions in mathematical form one can show readily that under these conditions $\dot{U}$, H, and Z must all be constants, which is a direct contradiction of the initial postulate that they are variables. By means, however, of the further inconsistency of treating H (*Hemmungsfaktor*) as if it were a constant in some operations and a variable in other operations, the authors arrive at the logarithmic equation given above.

Snoddy (34) also has recently suggested using a logarithmic function to represent learning data, stating that this equation is similar to one used in describing entropy.

In 1930 Thurstone (40) returned to the problem of the equation of the learning curve and developed a rational equation of the learning curve based on a more complete set of psychological assumptions than any hitherto advanced.

As far as can be determined, this was the first time a group of psychological assumptions regarding learning had been stated as

mathematical equations and certain observation equations derived rigorously from these initial postulates. Former equations of the learning curve were either empirical equations, chosen originally because of a resemblance between the learning data and the mathematical equation, or else were developed from a simple basic assumption expressed in one differential equation. One of the following two assumptions has been generally used: the rate of learning is proportional to the amount still to be learned, or the rate of learning is proportional to the product of the amount already learned and the amount still to be learned. Neither of these two assumptions has been thought of as related to the proof or disproof of any of the current psychological theories of learning. At least this relation has not been brought out in the work in which such assumptions have been used.

Thurstone assumes that "at any one moment of time all the acts which the learner is at all likely to initiate in his effort toward a goal may then be arbitrarily divided into two categories, those that lead to success and those that lead either to overt or implicit error. . . .

s equals total number of acts which the learner is likely to initiate and which would be credited as successes.

e equals total number of acts which the learner is likely to initiate and which would lead to error or failure or loss of time."

Two other variables, p and q, are defined,

"p equals probability that the act initiated at any moment of time will lead to successful completion.

q equals probability that the act initiated at any moment of time will lead to error or loss of time."

k is a constant representing the probability that, if an error is made, that error will be eliminated. Therefore, the probability at any moment that an error will be eliminated is kq. This quantity may also be regarded as the average number of errors eliminated each time the subject responds. This may be expressed by the differential equation

$$de/dt = -kq.$$

A new variable "t" representing time is introduced and it is assumed that acts, overt or implicit, are initiated at a constant temporal rate. Making analogous assumptions for the successful responses we may write the differential equation $ds/dt = kp$.

If it is assumed that each possible act is equally likely to occur, then

$$p = s/(s+e) \quad \text{and}$$
$$q = e/(s+e).$$

From these four equations, involving five variables, the functional relation between p and t can be determined. Thurstone shows this to be

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} + z.$$

Later Thurstone (41) used this same theoretical set-up to determine the functional relationship between number of trials and cumulative errors. This was done by assuming that u, the cumulative errors, and R, the number of trials, were related to the variables p, q, and t, as defined above, by the equations $adu=qdt$, and $dR=pdt$, where "a" is simply a factor to take care of the possible difference in the unit of measurement of q and u. The equation derived is

$$u = \frac{(\sqrt{m}/ak)\,R}{R + (\sqrt{m}/k)},$$

where u is cumulative errors, R is number of trials, and m and k are constants dependent on the task and the learner, respectively, as described above. This is the equation of a hyperbola which Thurstone suggested on empirical grounds in 1917 and 1919. However, in the hyperbola suggested in the earlier work, the variables were amount of work, per unit time and total time of practice, instead of cumulative errors and trials. Thurstone reports that this equation fits individual curves on rat maze learning data obtained by Dr. Lashley.

As stated above, certain propositions contained in the law of effect as recently stated by Thorndike (37) lend themselves very readily to statement in symbolic form and the development of an equation of learning which is implicit in them.

In his recent book, *The Fundamentals of Learning,* Thorndike suggests three methods of dealing with learning data to determine the stamping-in and the stamping-out effects of punishment and reward (or annoyers and satisfiers) in any given set of learning

records. The methods, while varying in details, all consist essentially in comparing the percentage error for all subjects in any one trial, with the percentage error for all subjects in the following trial, as, for instance, the errors in the second trial of all individuals with the errors in the third trial. It is necessary to group the results from a large number of subjects for each trial in order to have reliable percentages. Sometimes even finer subdivisions are suggested, such as the percentage making a correct response on the second trial, but not on the first trial, who also make an error on the third trial, and so on. These methods will be found discussed in detail in *The Fundamentals of Learning.*

Analyses of this sort are valuable for a preliminary exploration of data. There are, however, several objections to their use when an accurate evaluation is desired. First, they concentrate on the relationship between errors in one trial and the trial immediately following or preceding it, and so give no comprehensive picture of the entire course of the learning, a picture which it is desirable to have. The second and more important objection is that the method requires pooling the responses of a large group. When this is done it is probable that significant facts of learning are obscured by individual differences in learning rate and in initial attainment. The *general* case of individual versus average curves has recently been discussed by Merrell (24). She has given a general method for determining what kind of distortion, if any, will be produced by using the average instead of the individual curves for any function; and has shown that averaging will produce distortion in some functions, but no distortion in other functions. ". . . an average curve cannot be considered merely a reflection of the individual curves, since it may exhibit characteristics which are primarily due to the mathematical process of averaging and may not be present in the individuals" (24, p. 53). The objections presented above would not apply to the use of a rational equation of the learning curve derived from the law of effect. Such an equation would make it possible to fit the learning curve of a single individual and so not obscure or distort important facts by pooling the data from a large number of individuals.

The mathematical statement of certain parts of Thorndike's (37) Law of Effect proves to be a generalized form of the learning function developed by Thurstone (40, 41); therefore, in the present

development the symbols adopted by Thurstone have been used as far as possible in order to emphasize this relationship.

The assumptions of the law of effect which are to be expressed mathematically may be described as follows for some forms of learning (such as that which occurs in the typical discrimination box experiment). When the animal is first placed in the discrimination box situation, the connection between the stimulating situation and the correct response has a certain initial strength, and similarly the connection between the stimulating situation and the incorrect response has a certain initial strength. This corresponds to the reasoning in Thurstone's (40) paper where he points out that learning cannot occur unless both the correct and incorrect responses have some finite initial probability. This may be symbolized as follows:

h=the initial strength of the connection between the stimulating situation and the *correct* response.

g=the initial strength of the connection between the stimulating situation and the *incorrect* response.

h and g may then be said to represent the initial attainment of the organism in the experimental situation. (It can be seen that this set-up includes both the case where the animal's initial behavior is random with respect to the stimuli to be discriminated—i.e., the case where g equals h—and also the case where the animal from the very beginning shows a preference for either the correct or the incorrect response—i.e., where g is markedly larger or smaller than h. This is mentioned because some workers have stressed the fact that the subject's initial behavior is frequently not random behavior.)

Having taken care of the organism's initial attainment by these assumptions, it is necessary to include modifiability with practice. Thurstone did this by assuming that k represents the average number of new plausible correct responses introduced into the subject's repertoire by each correct response actually made. The corresponding assumption by Thorndike is that each time the correct response is repeated and rewarded the strength of its connection with the stimulating situation is increased by a certain constant amount. Symbolically this may be stated

k=the amount added to the strength of the correct response each time that response is repeated and rewarded.

It is clear then that after the first repetition of the correct response,

its strength is not h, but is $h+k$, and after the second repetition of the correct response its strength is $h+2k$ and so on, k being added to the strength of the correct response each time it is repeated and rewarded. This may be expressed generally by the equation

$$s = h + kw,$$

where h and k have already been defined, and where

s=a variable representing the strength of the correct response at any point during the experiment, and

w=a variable representing the cumulative count of correct responses.

The symbol s is used by Thurstone (40) and is there defined by him as "the total number of acts which the learner is likely to initiate and which would be credited as successes." The correspondence between this and the definition of s given above can readily be seen. The symbol w above corresponds to Thurstone's R. At the beginning of the experiment $w=0$ and $s=h$; after the first repetition of the correct response, $w=1$, and $s=h+k$; then after the third repetition of the correct response, $w=3$, and $s=h+3k$, etc.

The incorrect response can be dealt with in a similar manner. Each time it is repeated and punished the strength of its connection with the situation is diminished by a certain constant amount. Thurstone assumed that "the extent to which a subject profits by his errors is about the same as the extent to which he profits by his successes" and so used the constant k above to represent not only the average number of new or duplicate plausible correct responses introduced into the subject's repertoire by each correct response made, but also to represent the average number of errors eliminated each time a mistake is made. Thorndike, on the other hand, has presented evidence which he believes shows that the extent to which a subject profits by an error may be markedly different from the extent to which he profits by a success. Therefore in the present development, following Thorndike, the influence of error has been symbolized by

c=the amount subtracted from the strength of the incorrect response each time it is repeated and punished.

Two different constants c and k are used to allow for the possibility that the effect of a success may be markedly different from the effect of an error; whereas Thurstone assumed that $c=k$ and hence

used only one value k to represent "the learning constant of the subject." It follows that after the first repetition of the incorrect response its strength is not g, but is $g—c$, and after the second repetition of the incorrect response its strength is $g—2c$, and so on. In general,

$$e = g - cu,$$

where g and c have already been defined, and as with the correct response,

e=a variable representing the strength of the incorrect response at any time during the experiment, and
u=a variable representing the cumulative errors.

The symbol e is taken from Thurstone (40) and is defined by him as "the total number of acts which the learner is likely to initiate and which would lead to error or failure or loss of time." The correspondence between this and the above definition taken from Thorndike can readily be seen. The symbol u and its definitions is taken directly from Thurstone (41).

The above equations may be derived simply by stating the basic principle of the Law of Effect in the two differential equations.

$ds/dw=k$ (i.e., each repetition and reward of the correct response adds a certain constant amount k to the strength of the correct response) and
$de/du=-c$ (i.e., each repetition and punishment of the incorrect response deducts a certain constant amount c from the strength of the incorrect response.)

These two assumptions were stated by Thurstone (40) and were also given in non-mathematical form by Thorndike (37).

Integrating gives $s = h + kw$ and
$e = g - cu.$

The constants of integration are h and g. If $w=0$, $s=h$; if $u=0$, $e=g$; so it can be seen that the parameters and variables have the same definitions as above. (If the equation derived from this basic assumption is found not to fit data, it is possible that the assumption should be altered to some more complex form, such as

$$\frac{ds}{dw} = \frac{k}{w} \quad \text{and} \quad \frac{de}{du} = \frac{c}{u}.$$

Various other possibilities may suggest themselves, or be indicated by suitable experiments. These other basic postulates will give other forms of the equation of the learning curve which may be in better agreement with data, or more useful in interpreting data than the curve derived from the above assumption).

It can be seen that the equation $s = h + kw$ means that the strength of the correct response increases linearly with each repetition of the correct response. Plotting s against w would give a straight line with slope k and intercept h.

Correspondingly, the strength of the incorrect response decreases linearly with each repetition of the incorrect response according to the equation $e = g - cu$. Plotting e against u gives a straight line with slope $-c$ and intercept g.

In other words, it is assumed that the "learning curve" for a single response is linear. On a casual inspection this may seem quite unreasonable to many psychologists because most, if not all, experimental learning curves are non-linear. However, all experimental learning curves deal with a situation involving at least two possible responses—a correct and an incorrect response. That is, it is impossible to verify directly this assumption of a linear relationship between s and w and between e and u because it is impossible to measure directly either s (the strength of the correct response) or e (the strength of the incorrect response). Experimentally we can measure only the strength of s relative to e, or vice versa.

It is, therefore, necessary to find some relationship between these two variables. To do this Thurstone (40) and Thorndike (37) employed the concept of the probability of a correct response (designated by p). If Thorndike's "strength" be defined as relative frequency of occurrence, then at any time during the course of learning

$$p = \frac{\text{Strength of the correct response}}{(\text{Str. of correct response}) + (\text{Str. of incorrect response})}$$

$$\text{or } \frac{s}{s+e}.$$

This equation is taken from Thurstone (40) and is also given by Thorndike (37) in non-symbolic form. At the beginning of the experiment, for instance, $p = h/(h + g)$. Similarly, q is defined

as a variable which represents the probability of the incorrect response, then

$$q = \frac{\text{Strength of the incorrect response}}{(\text{Str. of correct response}) + (\text{Str. of incorrect response})}$$

$$\text{or } \frac{e}{s+e}.$$

This equation likewise is taken from Thurstone (40) and is also given by Thorndike (37) in non-symbolic form. At the beginning of the experiment $q = g/(h + g)$.

If the first response the animal gives happens to be a correct one, then the strength of the correct response is not h, but is $h+k$, while the strength of the incorrect response is still g. This, however, changes the probability of both the correct and incorrect response, so that $p = (h+k)/(h+k+g)$ and $q = g/(h+k+g)$; continuing in the same manner we may express the probability of the correct and of the incorrect response at any stage of learning. For instance, if in the first four responses to a situation, the animal makes two correct and two incorrect responses, then $p = (h+2k)/(h+2k)+(g-2c)$ and $q = (g-2c)/(h+2k)+(g-2c)$, and in general it can be seen that

$$p = (h+kw)/(h+kw)+(g-uc) \text{ and}$$
$$q = (g-uc)/(h+kw)+(g-uc);$$

dividing one by the other gives

$$q/p = (g-cu)/(h+kw).$$

By this procedure, the s and e which are not experimentally measurable have been eliminated from the equation, however the equation still has four variables in it, p, q, u, and w. If it is possible to reduce these to two variables, the verification will be much easier. In order to replace p and q by u and w it is necessary to use the relation

$$du/dw = q/p.$$

That is, if q is twice p then errors are increasing twice as fast as the correct responses; likewise if q is one third of p, that is if the probability of the organism's making an error is one third the probability that it will make a correct response, then the rate at which the errors are increasing will be 1/3 of the rate at which the correct responses are increasing.

Before proceeding with the rest of the derivation a summary of the above will be given, stating the six symbols with their definition and the five basic postulates. Then the complete derivation of an equation of the learning curve will be given.

Definition of Symbols

s = The strength of the connection between the situation and the correct response.

e = The strength of the connection between the situation and the incorrect response.

p = The probability of occurrence of the correct response.

q = The probability of occurrence of the incorrect response.

w = The total number of correct responses made up to any given time during learning (i.e., a running count of the number of correct responses).

u = Cumulative errors (total errors made up to any given time during the course of learning).

Basic Postulates

$$p = s/s+e \qquad [1]$$

$$q = e/s+e \qquad [2]$$

$$du/dw = q/p \qquad [3]$$

$$ds/dw = k \qquad [4]$$

$$de/du = -c \qquad [5]$$

Derivation of the equation of the learning curve

Integrating equations [4] and [5] gives

$$s = kw+h \qquad [6]$$

$$e = -cu+g. \qquad [7]$$

Dividing [2] by [1] gives

$$q/p = e/s. \qquad [8]$$

Substituting [8] in [3] gives

$$du/dw = e/s. \qquad [9]^2$$

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[2]It would also be possible to give a somewhat simpler derivation by regarding equation [9] as a postulate and substituting it for [1], [2], and [3], from which it is derived. This was not done partly because Thorndike stresses postulates [1] and [2] in his treatment of data, and partly because the relation of the present equation to Thurstone's rational equations is clearer with this statement. Thurstone (1930) derives an equation $p=f(t)$, which is a special case of the equation $p=F(t)$, which can be readily derived from the present set-up by inserting the two postulates with regard to time,

$$\frac{ds}{dt} = kp \quad \text{and} \quad \frac{de}{dt} = -cq.$$

Substituting [6] and [7] in [9] gives

$$du/dw = (g—cu)/(h+kw). \qquad [10]$$

Separating variables in [10] gives

$$\frac{du}{g—cu} = \frac{dw}{h+kw}. \qquad [11]$$

Integrating [11] gives

$$\frac{\log\ (g—cu)}{—c} = \frac{\log\ (h+kw)}{k} + \log\ j. \qquad [12]$$

In order to evaluate log j (the constant of integration) we make use of the fact that at the beginning of learning u and w both equal zero. When this is true equation [12] becomes

$$\frac{\log\ g}{—c} = \frac{\log\ h}{k} + \log\ j. \qquad [13]$$

Transposing [13] gives

$$\log\ j = \frac{\log\ g}{—c} - \frac{\log\ h}{k}. \qquad [14]$$

Substituting [14] in [12] gives

$$\frac{\log\ (g—cu)}{—c} = \frac{\log\ (h+kw)}{k} + \frac{\log\ g}{—c} - \frac{\log\ h}{k}. \qquad [15]$$

Changing coefficients of logarithms to exponents of the antilogs gives

$$\log\left[(g—cu)^{-\frac{1}{c}}\right] = \log\left[(h+kw)^{\frac{1}{k}}\right] + \log\left(g^{-\frac{1}{c}}\right) - \log\left(h^{\frac{1}{k}}\right) \qquad [16]$$

Transposing $\log\ (g^{-\frac{1}{c}})$ and taking the antilogs gives

$$\left(\frac{g—cu}{g}\right)^{-\frac{1}{c}} = \left(\frac{h+kw}{h}\right)^{\frac{1}{k}}. \qquad [17]$$

Taking the $—c$ power of both sides and taking the reciprocal of the

right-hand expression in parenthesis to make the exponent positive gives

$$\frac{g-cu}{g} = \left(\frac{h}{h+kw} \right)^{\frac{c}{k}} . \qquad [18]$$

Multiplying both sides by g/c, subtracting g/c from both sides, and then multiplying by —1 gives

$$u = \frac{g}{c} - \frac{g}{c} \left(\frac{h}{h+kw} \right)^{\frac{c}{k}} . \qquad [19]$$

Factoring out g/c and dividing the numerator and denominator of the expression in parenthesis by k gives the equation of the learning curve in its final form

$$u = \frac{g}{c} \left[1 - \left(\frac{\frac{h}{k}}{w + \frac{h}{k}} \right)^{\frac{c}{k}} \right] \qquad [20]$$

Where u and w are variables,

u representing cumulative errors, and

w representing cumulative correct responses.

(In certain experimental set-ups one trial is marked by one correct response. In these cases number of trials may be substituted for number of correct responses.)

h, g, c, and k are parameters of the equation, which means that for any one learning curve they are constants, although they may be markedly different for different learning curves.

g is the initial strength of the incorrect response,

h is the initial strength of the correct response,

k is the constant amount added to the strength of the correct response each time it is repeated and rewarded.

c is the constant amount subtracted from the strength of the incorrect response each time it is repeated and punished.

Thus g and h represent the individual's initial attainment in the task, while c and k represent his learning ability. Equation [20] gives the relationship between the cumulative correct and incorrect responses, initial attainment, and learning ability.

Various forms which this equation may take will now be presented. If g, h, c, and k are all positive, then as w increases without limit, u approaches $\frac{g}{c}$. Some of Thorndike's results, however, suggest that c may not be positive in all cases. In some experimental situations where the punishment is relatively weak, the stamping-in effect of a repetition of the incorrect response may exceed the stamping-out effect of the punishment used, so that the total effect of the repetition and consequent punishment of a wrong response may be a very slight strengthening of the incorrect response. What will happen to the learning curve if this holds throughout the course of the experiment, i.e., what form does equation [20] take if g, h, and k are positive, but c is negative and numerically *smaller* than k? The curve $u=f(w)$ will then be negatively accelerated and non-asymptotic. The total number of errors made will increase without limit as practice continues. This, however, does not mean that the subject will never learn the task in question. If the curve representing errors per trial (trial being marked by a complete correct response), is used, it is asymptotic to zero just as it is when the parameters are all positive, so that the subject's "errors per trial" can be reduced below any amount, however small. That is, with sufficient practice he will make less than an average of one-tenth of an error per trial, or less than an average of one-hundredth error per trial, and so on down. The type of curve obtained when the result of repetition and punishment of the incorrect response is to increase the strength of this response has been shown. The type of curve that would arise if repetition and punishment together had no effect at all may also be investigated. If c is zero, then equation [20] becomes indeterminate, and evaluation of this indeterminate expression by methods of calculus gives the equation

$$u = \frac{g}{k} \log\left(\frac{kw}{h} + 1 \right)$$

If in equation [20] we set $c=k$, then the present equation becomes identical with formula [30] in Thurstone (41).

Another special case of the present equation is also of interest. This is the case where the effect of a repetition and reward of the

correct response is very small. Mathematically stated, this is the case in which k approaches 0, or in which h/k and c/k approach infinity. Under these conditions, equation [20] becomes indeterminate, but it can be evaluated by methods of calculus and gives the equation

$$u = (g/c)\ [1 - e^{w(-c/h)}],$$

which can be seen to be an equation of the form called "type A" in the review of the literature given above. However, in the present case, the variables are cumulative correct responses and cumulative incorrect responses. The variables used by other writers have been different from these.

In fitting equation [20] to data, it is necessary to plot cumulative errors against cumulative correct responses. This method was suggested by Thurstone, who has shown that it may be used to smooth individual learning curves so that an equation may be fitted accurately to the learning curve of a single individual.

Using this method of smoothing, I have found that this equation fits my learning data for rats trained in visual discrimination by Lashley's jumping technique. The numerical value of the parameters was determined by using the rectified form of equation [20], which is

$$\log\ (g/c - u) = \log F - (c/k)\ \log\ (w + h/k)$$
$$\text{where } F = (g/c)\,(h/k)^{(c/k)}.$$

Trial values were used for g/c and h/k until values which gave a linear plot were found. This is not a rigorous method of determining the values of the parameters and so the fit shown here cannot be regarded as the best fit but only as an approximation. (Other methods of fitting this equation are being worked on.) The goodness of fit on data for seven rats is shown in Table 1 and also in Figure 1. Cases 11 and 12 are ones in which c equals k and so are special cases in which the formula developed here becomes identical with that suggested by Thurstone (41). Case 2 is one in which k approaches zero and so is the special case in which the function developed here takes the form designated as "type A" above.

Thurstone states that his equation, which is a special case of the present one, fits maze learning data for rats. The present equation has one more parameter than Thurstone's, and so would probably

TABLE 1

LEARNING DATA FOR SEVEN RATS TRAINED TO DISCRIMINATE AN 18-CM. FROM A 12-CM. CIRCLE

(Rats 1, 5, 11, and 13 were trained positively to the 18-cm. circle; rats 2, 10, and 12 were trained positively to the 12-cm. circle.)

For each value of w (cumulative correct responses or trials) given in the table the experimental and calculated values of u (cumulative errors) and the difference between them are given.

The values of the parameters g/c, h/k, c/k, and A are given for each curve.

(The data for rat 2 illustrate the special case in which the formula developed here becomes an exponential of type A. The parameters in this case are g/c and $-c/h$.)

		$-c/h = 0.021$	
A	—5	0	0
g/c	65	155	67
h/k	250	∞	75
c/k	4	∞	1.1

Trials (w)	Rat 1 Exp. u	Rat 1 Cal. u	Rat 1 Diff.	Rat 2 Exp. u	Rat 2 Cal. u	Rat 2 Diff.	Rat 5 Exp. u	Rat 5 Cal. u	Rat 5 Diff.
10	9	4.4	—4.6	27	29.3	+2.3	11	8.4	—2.6
20	12	12.2	+0.2	35	53.0	+18.0	13	15.3	+2.3
30	19	18.7	—0.3	70	72.2	+2.2	21	20.7	—0.3
40	23	24.1	+1.1	81	87.9	+6.9	24	25.1	+1.1
50	29	28.7	—0.3	94	100.6	+6.6	29	28.8	—0.2
60	31	32.4	+1.4	103	110.8	+7.8	31	31.9	+0.9
70	34	35.8	+1.8	115	119.2	+4.2	33	34.6	+1.6
80	40	38.6	—1.4	126	126.0	0.0	35	36.9	+1.9
90	41	41.0	0.0	131	131.4	+0.4	39	38.9	—0.1
100	46	43.1	—2.9	136	135.9	—0.1	41	40.6	—0.4
110	47	44.9	—2.1	141	139.5	—1.5	41	42.2	+1.2
120	47	46.5	—0.5	144	142.5	—1.5			
130	48	47.8	—0.2	146	144.8	—1.2			
140	49	49.0	0.0	148	146.8	—1.2			
150	51	50.1	—0.9	148	148.3	+0.3			
160	53	51.0	—2.0	148	149.6	+1.6			
170	53	52.0	—1.0	148	150.7	+2.7			
180	53	52.6	—0.4						
190	54	53.2	—0.8						
200	54	53.8	—0.2						
210	55	54.3	—0.7						
220	55	54.8	—0.2						
230	55	55.2	+0.2						
240	55	55.6	+0.6						

TABLE 1 (*continued*)

A		0			0			+17			
g/c		60			53			110			
h/k		30			22			24			
c/k		2.2			1.0			1.0			
	Rat 10			Rat 11			Rat 12			Rat 13		
Trials (w)	Exp. u	Cal. u	Diff.	Exp. u	Cal. u	Diff.	Exp. u	Cal. u	Diff.	Exp. u	Cal. u	Diff.
10	23	28.1	+5.1	17	16.5	—0.5	24	49.4	+25.5	22	24.7	+2.7
20	43	40.5	—2.5	26	25.2	—0.8	67	67.0	0.0	39	38.1	—0.9
30	48	46.9	—1.1	30	30.6	+0.6	77	78.1	+1.1	44	46.4	+2.4
40	50	50.7	+0.7	34	34.2	+0.2	87	85.8	—1.2	49	52.0	+3.0
50	51	53.0	+2.0	35	36.8	+1.8	94	91.3	—2.7	57	56.1	—0.9
60	53	54.7	+1.7	40	38.8	—1.2	100	95.6	—4.4	59	59.2	+0.2
70	56	55.7	—0.3	41	40.3	—0.7	100	98.9	—1.1	63	61.6	—1.4
80	56	56.6	+0.6	42	41.6	—0.4	101	101.6	+0.6	64	63.5	—0.5
90				42	42.6	+0.6	106	103.8	—2.2	67	65.0	—2.0
100							107	105.7	—1.3	67	66.4	—0.6
110							107	107.3	+0.3	67	67.5	+0.5
120							107	108.7	+1.7	67	68.4	+1.4
130							107	109.9	+2.9			

A	0
g/c	80
h/k	25
c/k	1.1

fit the maze data as well as, or better than, his equation. The fit to maze data is interesting because the maze situation seems to be much more complex than that given in the theoretical set-up from which this equation was derived.

In fitting learning data for a single individual on a single task the numerical values for g, h, c, and k cannot be determined, but only the ratios between these values. The same is true of Thur-

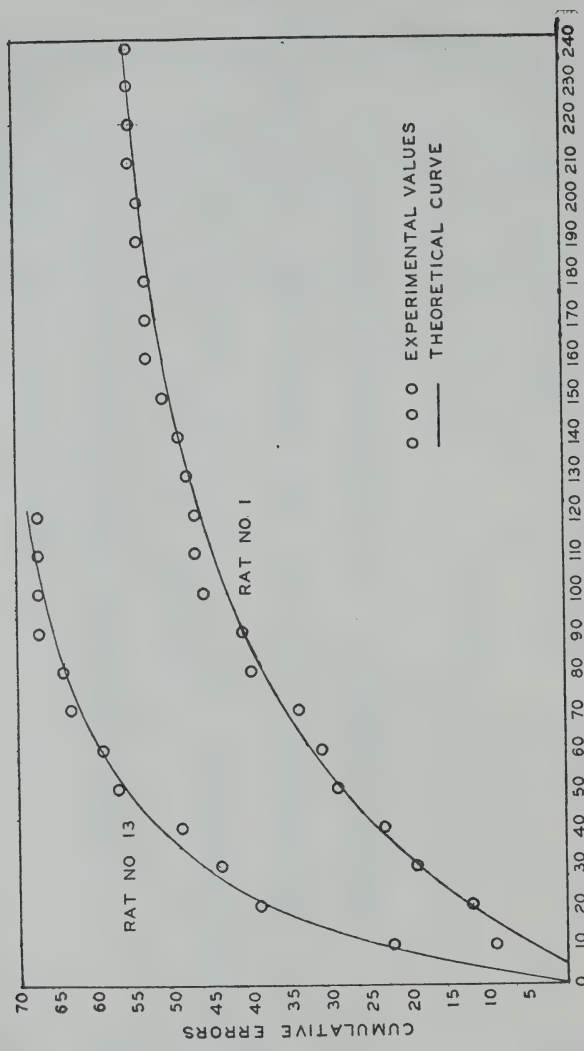

FIGURE 1

INDIVIDUAL LEARNING CURVES FOR TWO RATS

stone's parameters m and k. He has suggested that the numerical values of these constants may be determined by using each one of a group of subjects on each of a number of tasks (40, p. 479). Moore (27, p. 200) expresses the opinion that this method is too laborious to be valuable. The importance of the problem justifies the labor, however. With the present function, another method of determining the numerical values of c and k may be useful. It is possible to determine what the learning curve would have been if each of the subjects had started at the point where g/h equals 1, or where the strength of the correct and incorrect responses are equal. With this method comparable values for the individual learning constants c and k may be obtained for different subjects. Further work on the function is needed to determine just what method of comparing learning ability for different individuals is most fruitful.

This equation is presented here as being developed from Thorndike's law of effect. However, it should be noted that there are several theories (Hobhouse, Holmes, Carr) which are, as Tolman (42) points out, different formulations of the law of effect. These other formulations are like Thorndike's in that they assume that initially the stimulus may give rise to any one of a number of responses and that gradually certain responses are strengthened and others are weakened. These theories differ from Thorndike's in that they assert that different factors are responsible for the stamping-in and the stamping-out (Thorndike—satisfiers, annoyers; Carr —successful or unsuccessful sensory consequences, etc.). It can be seen that the mathematical formulation does not state *what* stamps in, or *what* stamps out, but merely *that* there is such a process. It is even possible that the formulation underlying this equation would apply to other theories of learning that seem superficially to be markedly different from the Law of Effect. The present formulation may even apply to Tolman's recent theory that learning does not consist in the formation of stimulus response bonds but rather in the formation of "sign-gestalt-expectations." This cannot be determined until there has been a more definite formulation of the theory.

As pointed out in the introduction, the method of mathematical hypothesis has not been generally applied to learning problems. Perhaps this is due to the belief that there are certain objections to this procedure in the case of learning. It is sometimes said that

the phenomena of learning are so complex, so sensitive to the influence of many different factors, that it is impossible to include all of these factors in an equation. Undoubtedly this is true. A large number of experiments on the different factors influencing learning could be cited to prove this. However, as Thurstone (40) has pointed out, in order to develop rational equations for learning it is necessary to oversimplify the phenomena. This scientific oversimplification is not peculiar to the field of learning. Hecht (13) has pointed out that it is also necessary in dealing with sense-organ phenomena. The same sort of oversimplification is necessary in dealing with physical processes and is one of the characteristics of science. Slight changes of resistance of a wire (with normal variations in temperature) are ignored in mathematical treatment of simple electrical problems. Similarly in describing learning—especially in the initial attempts to formulate quantitative statements—it will be necessary to oversimplify the theoretical treatment and then to control the experimental situation so that the effect of the variables which have been ignored is reduced to a minimum. Hecht (13) maintains that simplification is best even in cases where it is not necessary because it gives a clearer formulation of the essentials of the problem, and errors in the theory can be detected more easily. It is granted then that the phenomena of learning are tremendously complex and that any mathematical treatment (especially in its initial stages) will oversimplify the facts. However, it cannot be said that therefore the method of mathematical hypothesis cannot be applied to learning. In fact, some degree of oversimplification is found in most if not all scientific formulations. This over simplification is not a peculiarity of mathematical treatment of a theory. It applies also to non-mathematical treatment, which in addition to being oversimplified, usually cannot be as rigorous and accurate as the mathematical treatment.

It may be pointed out that the equation of learning given here seems to be fairly adequate to describe the learning of rats in at least two different types of situations, yet there are many factors which undoubtedly influence learning that were omitted in deriving the present equation. Some of these omissions may be enumerated here in order to give some concrete suggestions for further refinement of the mathematical treatment of learning. (1) The law of disuse has not been included. It seems probable when the correct response is repeated a few times in succession, the strength of the

incorrect response, instead of remaining constant (as is assumed here), is weakened through disuse. The same perhaps holds true for the correct response when the subject makes a number of errors in succession. (2) The theoretical formulation does not take into account variations in time between presentation of different parts of the series, and hence leaves out all consideration of the effects of "distributed practice." (3) Likewise, no theoretical formulation that would cover the facts of whole *vs.* part learning has been included. (4) Also the theory as stated here implies that the probability of any error will gradually decrease, approaching, but never reaching, zero. There are many illustrations of tasks so difficult that the probability of making an error will not approach zero as a limit but will approach some value like $\frac{1}{5}$ or $\frac{1}{10}$. Target-shooting would be an illustration of this, if the target were made very small and put at a great distance. The percentage of misses would decrease and probably would approach some fraction larger than zero. This will be the case in all tasks that are relatively difficult for the organism, tasks in which the correct and incorrect responses are rather "unavailable," to use Thorndike's term. An attempt to apply the present equation to various types of data will doubtless reveal still other factors that have been omitted. Development of the mathematical treatment to include these and other factors may prove to be very difficult. However, the fact that, despite these omissions, the equation still describes certain types of data rather well would seem to indicate that considerable progress is possible in describing learning in quantitative terms.

The statement is sometimes made that an equation of the learning curve cannot be formulated because the method and course of learning varies so markedly from subject to subject. That is, one subject may learn rapidly, while another subject learns slowly, and still another subject may not learn the task at all. The above constitutes an argument against plotting some sort of average or group learning curve [a common practice which is also criticized by Merrell (24)]. If the different subjects are markedly different it seems reasonable to state that it is rather misleading to attempt to plot, and subject to mathematical treatment, a learning curve for the group. However, if a separate learning curve is plotted and fitted

for each subject it can be seen that the fact that one subject is different from another subject constitutes no objection to mathematical treatment of the learning curve.

It has also been pointed out that the learning curve is different for different types of tasks. There have been many investigations of different factors which may influence the shape of the learning curve (see McGeoch, 20, 1929, p. 459; 21, 1930, p. 514; 22, 1931, p. 414; 23, 1933, p. 13). Some investigators find positive acceleration, some negative, and some find *s*-shaped curves depending partly at least on the type of learning set-up used. But to conclude from this that it is useless to attempt any mathematical treatment of learning is to overlook the fact that each variation of the experimental conditions must be accompanied by corresponding alterations in the theoretical set-up. This possibility of transforming one form of a rational equation into other forms (to take care of differences in the experimental situation) emphasizes the difference between rational and empirical equations. Empirical equations cannot be transformed in this way, while rational equations can. It is also clear that if different variables are used in plotting the learning curve corresponding changes must be made in the equation used to fit the data. This is an obvious point and has been emphasized by Thurstone (39, p. 41) and Valentine (44, p. 421), but it is sometimes forgotten.

In general, any equation should not be applied indiscriminately to any learning data, just to see if it fits. The equation and the experimental set-up must be examined to see that the experimental set-up from which data are gathered conforms to the theoretical set-up from which the equation is derived and that the variables used in plotting the experimental learning curve are the same as the variables in terms of which the equation is written. Thus, equation [20] above may be used only on data in which the number of wrong responses and the number of correct responses can be counted, and in which the learning curve is plotted by graphing the total number of errors at any time against the total number of correct responses (which in many set-ups is the number of trials). The equation has been developed for the case where there is only one correct response and one incorrect response. In any serial learning such as a series of nonsense syllables, or a series of paired associates, this equation probably should not be used, unless the data

were analyzed and the curves plotted separately for each response that had to be learned. The formula, however, could be used in a discrimination box set-up where there was one correct doorway and a great many incorrect ones, or for a set-up where several doorways were correct and several incorrect.

In summary, the above objections to mathematical treatment of learning do not apply if three facts are borne in mind:

1. In any scientific field a mathematical formulation may involve oversimplification of the facts.
2. Individual and not group learning curves must be plotted.
3. An equation cannot be applied to data, regardless of the meaning of the equation. This includes both the meaning of the specific variables involved in the equation, i.e., the coordinates used, and also the postulates in the original theoretical set-up from which the equation was derived.

In the further use of the present equation attempts should be made to apply it to a variety of other types of data to see if it fits within the limits of experimental error. This verification has been made with only the two types of data mentioned and the criterion of learning was only 10 or 30 consecutive errorless trials. In order to determine more accurately how closely this equation fits data the animals should be run for a very long time, say to a criterion of 300 consecutive errorless trials. Then it would be possible to see how accurately the asymptote predicted from the first part of the curve agreed with the one which was finally approached by the animal.

"Goodness of fit," however, is only one test of the scientific usefulness of an equation. It may happen that several different equations fit a given set of data equally well. Valentine (45, 46) and Ettlinger (7) have pointed out that in certain cases the hyperbole, the arc-cotangent, and two different "growth" curves can be made to approximate each other closer than any one of the curves will probably approximate the learning data. This means that in such cases the goodness of fit of an equation to data can never be the final means of deciding between two different equations. Another test, however, can be applied which is independent of the goodness of fit test. The scientific use which can be made of the parameters of an equation is just as important as goodness of fit. This type of test has not been generally emphasized in dealing with equations of the learning curve.

It was stated above that in fitting learning curves the equation must be fitted independently for each subject, for each task. This means an independent determination of the parameters in each case. This must be done in the initial stages of application of a given equation, but if it is found by using the equation that the parameters are actually independent for each subject and each task, then the equation is of little use. But, as Thurstone (40) has pointed out, if it is found, for instance, that the parameter representing the animal's learning ability remains constant for a given animal for different tasks, or varies in a certain predictable way from task to task, then after the animal has been tested on a few tasks one of the parameters of his learning curve can be predicted for new tasks. Also, if it is found that another parameter of the equation varies from task to task, but is constant for all animals for a given task (or varies in some predictable way from animal to animal), then it can be seen that the equation becomes a valuable means of predicting the learning curve for any animal in any given task, provided that the given animal has been tried out in other tasks, and other animals have been tried in the given task. It is only if some use such as the above can be made of an equation that it is of real predictive value. That is, not only the goodness of fit must be tested, but the usefulness of the parameters must also be tested. The exact method of doing this depends partly on the parameters and their interpretation, and partly (perhaps mostly) on the ingenuity of the investigator. No set rules can be laid down for this test, as can be done for the tests of goodness of fit.

The particular value of the present equation is that the evaluation of its parameters gives a quantitative expression for initial attainment and for learning ability of a single individual in a single task. It will be remembered from the development of the equation that the parameters h and g represent initial attainment and c and k represent learning ability. Some problems will be suggested where this distinction may be useful. (See discussion of parameters on pages 416-417.)

First the experiments on maturation versus learning may be considered. The general method of such experiments may be illustrated by reference to the early work of Shepard and Breed (33) on pecking in chicks. One group of animals was allowed to peck at food each day for two weeks, beginning when they were one day old. A

record was kept of their accuracy. A second group was kept away from light, fed artificially for the first 3 to 5 or 6 days, then allowed to peck, and their accuracy recorded. It was found that the first group improved slowly, approaching a certain limit, and the second group also improved toward the same limit, but much more rapidly. The initial attainments of the two groups were much lower than their final ability, but it is not clear that they were equal. If it is found that the present equation of the learning curve is adequate to describe the data, then both sets of data can be analyzed to determine the parameters g and h, representing initial attainment, and parameters c and k, representing learning ability for each animal. Then by comparing the parameters of the animals in one group with the parameters of the animals in the other group, it would be possible to determine to what extent maturation affected initial attainment and to what extent it affected the organism's ability to learn.

The possibility presented here of distinguishing analytically between an animal's initial attainment in a given task and his learning ability suggests a certain analysis of the mechanism of transfer. Suppose it is found that after training on habit A a group of animals will learn habit B significantly faster than another group of animals who have not had the initial training on habit A. It can then be said that the training on habit A has improved the learning of habit B. But how has it improved this performance? The present equation suggests two possible answers, or some combination of these two. One possibility is that the training on the stimuli used in habit A has actually changed the animal's tendency to react to the stimuli used in habit B (i.e., changed g or h). Another possibility is that the training on habit A has not changed the animal's tendency to react to the stimuli used in habit B, but instead has changed his ability to profit by experience (i.e., altered c or k). The third possibility is that *both* the initial attainment, as represented by g and h, and the learning ability, as represented by c and k, have changed to some extent. Again in this case, by applying the present equation, the amount and kind of such change in each of the variables would be ascertained. The above discussion illustrates one question regarding transfer which may be answered by the mathematical analysis of data made possible by the present equation.

The present equation also makes possible certain refinements in the

attack upon the problem of intelligence. Intelligence is sometimes defined as learning ability, or the ability to profit by experience. However, learning ability seems to be a difficult thing to measure with sufficient accuracy. One difficulty is that, in any given task, the animal's "learning score" (whether time, errors, or trials, or some other measure) is influenced not only by the ability to learn, but also by initial attainment. If the animals have had previous experience with other tasks, then the factors of transfer, positive or negative, complicate the picture. Even if the animal has had no previous experience, the errors made during the process of adaptation to the learning situation complicate the picture. It is possible through the use of the present equation to correct each animal's learning records for differences in initial attainment. That is, it is possible after fitting the equation to the data to consider the learning curve as beginning at the point on the curve where the strength of the correct and incorrect responses are equal. This means that one can get an estimate of what the g/c and h/k values would have been for each of the animals if they had started with no tendency to prefer either the correct or the incorrect response. Such a correction for individual differences in initial ability has not been possible heretofore. Such a correction can be made on the records of each animal in each task, giving what may prove to be a more accurate estimate of the learning ability of the animal. Using this corrected value, the constancy of the learning ability of the animal in different tasks can then be tested, and also the correlation of learning abilities in different tasks.

If by the use of this new method on animals a constancy of learning ability is shown which is not shown by the more usual methods of taking some gross measure of learning ability such as total errors, then an entirely new field of investigation will be opened up for studies on human intelligence, increases in learning ability with maturation, etc.

Work in the field of forgetting can perhaps also be dealt with by this same technique. In this case one would allow considerable forgetting to take place, and then have the subject relearn. The second curve could then be compared with the original learning curve both as to initial attainment and learning ability. And the constants for initial attainment in the second curve could be compared

with the values for final ability in the first curve to see if there had been a certain fading of the effects of the first learning.

Work on degrees of overlearning are at present very unsatisfactory because there is no clear criterion for measurement of degree of overlearning, but according to Thorndike's assumption the strength of the correct response is still increasing very markedly even when the learning curve is very close to its asymptote. It may be that this will help to explain the great persistence of some sorts of much overlearned tasks.

In comparing the work of different investigators on learning, one question that is continually coming up is "Was the amount of training the same in the two cases?" One investigator will use one criterion of learning (such as 10 consecutive errorless trials) and another will use another criterion (such as 18 trials correct out of the last 20, etc.). The present equation furnishes a means of estimating the "degree of learning," basing that estimate upon the entire learning curve and not simply upon the animal's performance during the last 10 or 30 trials. This method is to state the slope of the learning curve at the point where training ceased. In comparing data from two experiments, the degree of learning in each case could be stated in terms of the slope of the curve at the time when training ceased.

Summary

1. Certain propositions contained in Thorndike's Law of Effect have been stated in mathematical form, and it has been shown that these same functions may be obtained by generalizing the learning theory recently given by Thurstone. From these functions an equation of the learning curve giving the relation between cumulative errors and cumulative correct responses has been developed. This equation is:

$$u = \frac{g}{c}\left[1 - \left(\frac{\frac{h}{k}}{w + \frac{h}{k}}\right)^{\frac{c}{k}}\right],$$

in which u = the cumulative errors,
w = the cumulative correct responses,
g = the initial strength of the incorrect response,

h = the initial strength of the correct response,
c = the constant amount deducted from the strength of the incorrect response each time it is repeated and punished,
k = the constant amount added to the strength of the correct response each time it is repeated and rewarded.

2. The equation given here is more general than Thorndike's Law of Effect because the mathematical formulation does not state *what* stamps in or *what* stamps out, but merely that there are two such processes, and that they each proceed at their own constant rate. Therefore, the present formulation may apply to theories which in some aspects are markedly different from Thorndike's theory.

3. In the special case where k approaches zero, the function becomes an exponential (called "type A" above) which has been suggested as an equation of the learning curve by several writers. However, in the present case, the variables are cumulative correct responses, and cumulative incorrect responses. The variables used by other writers have been different from these.

4. A rational equation should be applied to learning data only when the experimental set-up from which the data are taken agrees in general with the theoretical set-up from which the equation was derived. The present equation should be used only for experimental set-ups where a single situation is repeatedly presented to the subject and calls forth initially two or more responses, one or more of which are consistently "correct" and the rest consistently "incorrect." The equation should be used only for the curve which is plotted by graphing cumulative correct against cumulative incorrect responses.

5. This equation fits data for visual discrimination in rats trained by Lashley's jumping technique, and Thurstone reports that his special case of this equation fits data for maze learning in rats. So far, this equation has not been tried on other data.

6. The use of the slope of the learning curve at the point where practice ceased was suggested as a more uniform and more comparable criterion of "degree of learning" attained by the individual than any of the variety of criteria now in use.

7. In addition to goodness of fit, the scientific usefulness of the parameters of an equation must be tested. By means of the parameters of this equation, it is possible to distinguish analytically be-

tween initial attainment and learning ability of a single subject in a given task. This distinction may be valuable in the study of various psychological problems such as maturation versus learning, transfer, intelligence, forgetting, and overlearning. The use of the learning constants of this equation, or some function of them, to represent intelligence may prove to be a fruitful approach to this problem.

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Mooseheart Laboratory for Child Research
Mooseheart, Illinois

UNE ÉQUATION RATIONNELLE DE LA COURBE D'APPRENTISSAGE BASÉE SUR LA LOI DE L'EFFET DE THORNDIKE

(Résumé)

On donne en forme mathématique certaines propositions contenues dans la Loi de l'Effet de Thorndike, et l'on montre que l'on peut obtenir les mêmes fonctions en généralisant la théorie de l'apprentissage récemment donnée par Thurstone. On développe de ces fonctions une équation de la courbe d'apprentissage laquelle donne la relation entre les erreurs cumulatives et les réponses correctes cumulatives. Cette équation est plus générale que la Loi de l'Effet de Thorndike parce que la formulation mathématique ne dit pas *ce qui* fait entrer et ce qui fait sortir, mais seulement qu'il y a deux processus de cette nature, et que chacun procède à sa propre vitesse constante. Donc, la formulation que voici peut s'appliquer à des theorie qui sont à quelques égards très différentes de la théorie de Thorndike.

Cette équation s'adapte aux données pour la discrimination visuelle chez les rats.

On suggère l'emploi de la pente de la courbe d'apprentissage au point où l'exercice a cessé comme critère plus satisfaisant du "degré d'apprentissage" atteint par l'individu, que n'importe lequels des critères maintenant employés.

Au moyen des paramètres de cette équation, il est possible de distinguer analytiquement entre le rendement initial et la capacité d'apprendre d'un seul sujet dans une tâche donnée. On discute la valeur de cette distinction pour la résolution de différents problèmes psychologiques.

GULLIKSEN

EINE RATIONELLE GLEICHUNG DER LERNKURVE, DIE AUF THORNDIKES GESETZ DER WIRKUNG BERUHT

(Referat)

Gewisse Behauptungen, welche in Thorndikes Gesetz der Wirkung (law of effect) enthalten sind, werden in mathematischer Form gegeben, und es wird gezeigt, dass dieselben Funktionen durch Verallgemeinerung

der Lerntheorie, die neulich von Thurstone aufgestellt worden ist, abgeleitet werden können. Aus diesen Funktionen wird eine Gleichung der Lernkurve, welche die Beziehungen zwischen anhäufenden Fehlern und anhäufenden richtigen Antworten angibt, abgeleitet. Diese Gleichung ist allgemeiner als Thorndikes Gesetz der Wirkung, weil die mathematische Formulierung nicht angibt, was eingeprägt oder verlernt wird, sondern bloss dass es zwei solche Vorgänge gibt, und dass sie zu ihrer eigenen beständigen Schnelligkeit ablaufen. Daher dürfte diese Formulierung auf Theorien angewendet werden, die gewissermassen verschieden von Thorndikes Theorie sind.

Diese Gleichung eignet sich für Daten der visuellen Unterscheidung bei Ratten.

Der Gebrauch der Neigung der Lernkurve, wo Uebung aufhörte, wird als ein besseres Kennzeichen des Grades des Lernens, den ein Individuum erreicht, als irgendein anderes Kennzeichen, das jetzt gebraucht wird, vorgeschlagen.

Vermittellst der Parameter dieser Gleichung ist es möglich, zwischen Anfangsfähigkeit und Lernfähigkeit des einzelnen bei einer gegebenen Aufgabe analytisch zu unterscheiden. Der Wert dieser Unterscheidung zur Lösung verschiedener psychologischer Probleme wird erörtert.

GULLIKSEN

SHORT ARTICLES AND NOTES

THE SENSITIVITY OF VARIOUS AREAS OF THE BODY TO VIBRATORY STIMULI

LOUIS D. GOODFELLOW

The work of Professor Robert H. Gault and his research staff on the interpretation of speech and music by tactual and vibratory cues frequently brings forth the question of the relative sensitivity of different areas of the skin to mechanical vibrations. The same question, phrased differently, comes also from teachers of the deaf who are using the Gault-Teletactor.[1] They ask, "Through what area of the skin can a person most successfully receive speech and music communicated to him by means of the Gault-Teletactor?" As a tentative answer to these queries, we submit the following data.

I. THRESHOLDS FOR VIBRATORY STIMULI

The apparatus for the measurement of sensitivity consists of a Western Electric 2-A Audiometer, an amplifier, and a teletactor receiver unit. The intensity control dial of the audiometer is calibrated to read in decibels[2] above the threshold of the normal ear when used with the proper head phone. When using the audiometer to obtain vibratory thresholds, we take as our zero level the theoretical threshold of the normal ear for tones produced by our "standard" teletactor receiver unit. Thus when the intensity control dial is adjusted so that the observer can just barely feel the vibrations, the reading on the dial (plus the necessary corrections) indicates the sensitivity of the particular area of the body to vibratory stimuli in decibels above the theoretical threshold of the normal ear to tones produced by the same apparatus. In other words, our technique for determining a vibratory threshold virtually amounts to this. Holding our "standard" teletactor receiver unit to a normal ear we adjust the audiometer to the threshold of

[1]The Gault-Teletactor consists of a microphone, an amplifier and a receiver unit (sometimes referred to as the vibrator) upon which the deaf person places a finger to receive the vibrations of the speaker's voice. For a description of the apparatus see Goodfellow and Krause (4).

[2]A decibel is a measurement of the intensity level of sound above or below another level which in this case is the threshold of the normal ear. It is defined as twenty times the common logarithm of the ratio of the two sound pressures. It may help the reader to note that a difference of one decibel means that the sound pressure (intensity) has changed about twelve per cent. Five decibels means a change of seventy-eight per cent, ten decibels represents a change of 217 per cent, and twenty decibels a change of tenfold.

hearing. Then we have a deaf person place his finger-tip on the teletactor unit and find this threshold. The difference between the auditory and vibratory thresholds represents the sensitivity of the finger-tip. Other areas of the body are measured similarly, using the normal ear always as the reference point.

Instead of determining the auditory threshold for normal hearing observers, we used the accepted thresholds (established by Dr. Harvey Fletcher) corrected for our apparatus. This value is the zero point for all the following data.

A modified form of the Just Noticeable Difference Technique was employed in determining the thresholds. The intensity dial was adjusted so as to produce a vibration which was easily detected. Then the intensity was reduced until the observer was no longer able to feel it. The threshold was then approached from below by rotating the intensity control dial in the opposite direction until the observer reported that he detected a vibration. This process was repeated six times and the average value for the descending and ascending series was taken as the vibratory threshold for that particular area of the body. As was stated before, this figure represents the vibratory threshold in decibels above the threshold of the normal ear. Thresholds were obtained for thirteen different areas of the skin and at 64, 256, and 1024 d.v./sec. Table 1 gives these data based on two totally deaf and four normally hearing individuals.[3]

Probable errors have been computed and they all lie between 0.3 and 0.8. Consequently, a difference in sensitivity of five decibels or more may be considered statistically significant.

Probably a more significant index of the reliability of the above figures is an observer's consistency on a second test. Twelve observers, for whom we had made tactograms[4] for another experiment, were tested again after a period of two months or more, and a study of the change in sensitivity showed the average to be 3.5 decibels. In no case was a change greater than seven decibels found.

It is interesting that the thresholds here obtained for the different frequency levels are in fairly close agreement with those found in a previous and more intensive study of the relative sensitivity of the finger-tip and ear (3), in which different apparatus and different observers were used. As to the relative sensitivity of different areas at any one frequency, the dif-

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[3]In the case of normally hearing observers, all possible auditory clues were eliminated by a sound barrier, i.e., a pair of earphones worn by the observer in which a fairly loud tone of the same pitch as the one being used in the experiment was maintained throughout.

[4]A "tactogram" is a coined word corresponding to the audiogram, and is a record of a person's sensitivity to vibratory stimuli at various frequency levels.

TABLE 1

THE SENSITIVITY OF VARIOUS AREAS OF THE BODY TO VIBRATORY STIMULI IN DECIBELS BELOW THE SENSITIVITY OF THE NORMAL EAR. (The figures in parentheses indicate the order of sensitivity.)

Area of body	Frequency level 64		256		102		Touch spots per sq. cm.*
1. Left index finger	(1)	40	(2)	67	(2)	95	100
2. Right index finger	(3½)	44	(5½)	75	(4)	104	100
3. Right index finger (wearing kid glove)	(6)	48	(8)	78	(7)	108	100
4. Right index finger. Using sharp point instead of large conical diaphragm	(3½)	44	(5½)	75	(5½)	105	100
5. Back of right index finger	(5)	45	(4)	74	(5½)	105	9
6. Nail of right index finger	(2)	43	(3)	70	(1)	92	
7. Tip of tongue	(13)	59	(13)	90	(11)	118	
8. Sole of foot	(12)	56	(9)	77	(8½)	114	
9. Palm of right hand	(11)	54	(1)	66	(3)	101	119
10. Inside surface of right forearm	(9½)	53	(11)	80	(12)	121	23
11. Inside surface of right arm after shaving hair	(9½)	53	(10)	79	(13)	124	23
right forearm	(7)	49	(7)	76	(8½)	114	
12. Outside surface of							
13. Outside surface of right arm after shaving hair	(8)	52	(12)	83	(10)	119	

*For authority see von Frey (2), and Stroghold (6).

ferences represented in the above table are not highly significant, save the following:

1. The tip of the tongue is relatively insensitive to vibratory stimuli compared to its great sensitivity to touch.

2. There seems to be no correlation with the distribution of sensory spots. For example, the back of the finger is just about as sensitive as the palmar surface, even though it contains only about one-tenth the number of touch spots.

3. The sensitivity of the finger-tip is not greatly decreased by wearing a kid glove.

4. The outside surface of the forearm is slightly more sensitive than the inside surface.[5] After shaving the hair off the sensitivity is decreased

[5]When this part of the experiment was planned, we thought we had evidence of a difference in nerve supply to the two surfaces of the forearm

slightly on the outside surface, while the inside surface remains the same as before shaving.

5. The finger-tip of the right hand is less sensitive than that of the left hand. It has been suggested that, since all of our observers were right-handed, this decreased sensitivity might be due to a more calloused skin brought about by greater use. Such an interpretation is not consistent with the fact that a kid glove worn by our observers did not decrease sensitivity to vibratory stimuli. It is interesting to compare this finding with that of Smith (5) in which he found greater sensory acuity in the non-preferred hand.

6. The relative sensitivity of different areas is fairly consistent at the different frequency levels, except in the case of the palm of the hand, which is fairly insensitive to low frequencies and relatively sensitive to the high ones.

II. Discriminatory Capacity of Various Areas of the Body

As to the relative efficiency of various areas of the skin in detecting and interpreting various elements of speech, a small amount of data is available. For this study two totally deaf individuals, two normally hearing

TABLE 2

Table Showing the Degree of Efficiency in Percentage for Various Areas of the Skin in the Detection and Interpretation of Different Elements of Speech by Their Tactual Correlates. (The figures in parenthesis represent the rank-order of different areas.)

Area used	Identification of 5 vowel sounds		Detection of pauses		Detection of accent		Recognition of sentences
Right index finger	(2)	72	(6	80	(3½)	84	58
Right index finger (using sharp point instead of large diaphragm)	(4)	61	(4)	82	(5½)	72	51
Left index finger	(1)	76	(1)	88	(1½)	87	63
Palm of right hand	(6)	55	(2)	86	(3½)	84	43
Right index finger (gloved)	(7)	50	(7)	79	(7)	57	
Back of right index finger	(5)	60	(5)	81	(5½)	72	
Nail of right index finger	(3)	70	(3)	85	(1½)	87	

which might lead one to expect to find a difference in sensitivity. However, we have not been able to verify this evidence. Nevertheless, we include the data on sensitivity, although we cannot correlate it with neurological data at the present time.

persons who had had experience with the Teletactor, and three naïve hearing persons observed. Efficiency with the Teletactor, using different areas of the skin, was measured for four different types of tasks, namely: the recognition of vowel sounds, the detection of emphasis in speech, the detection of the pauses in speech, and the learning of sentences by their vibratory patterns.

In order to keep the stimuli constant, the above-mentioned materials were recorded on phonograph records which were used throughout the experiment. The area of the skin being used was changed every few minutes so as to equalize as much as possible the effects of practice, fatigue, etc., on the different areas being compared. The results are summarized in Table 2.

There seems to be little relation between sensitivity of an area and success with the Teletactor. For example, Table 2 shows the back of the finger to be much inferior to the tip in the actual use of the Teletactor, although Table 1 shows that the back of the finger is just about as sensitive as the tip. Further evidence pointing to the same conclusion may be found in Table 3, giving our observers' rank-order in sensitivity and actual success

TABLE 3

TABLE SHOWING THE SENSITIVITY AND DISCRIMINATORY CAPACITY OF SEVEN OBSERVERS

Observer	Rank in sensitivity	Rank in discriminatory capacity
A	7	6
B	5	5
C	6	3
D	2	7
E	4	2
F	1	4
G	3	1

with the Teletactor in the detection and interpretation of speech and music.

This apparent lack of relationship between sensitivity and success with the Teletactor can probably be explained on a basis of differences in intelligence, interest in the experiment, and personality factors, although it should be mentioned that all observers were of the college level in intelligence and had all volunteered their services—thus indicating some degree of interest in the experiment.

In summarizing, we point out the following conclusions suggested by experimental findings reported in this paper.

1. A wide range of differences in sensitivity exists for different areas of the skin. For example, the energy of a vibrating receiver which can be felt by the finger-tip has to be increased over twenty decibels, or in the lay-

man's language, over tenfold, before it is perceptible to the tip of the tongue.

2. The change in sensitivity over a period of two months is within the probable error of the experiment.

3. There appears to be no relation between a person's degree of sensitivity to vibratory stimuli and his ability to discriminate between elements of speech communicated to his fingers by the Teletactor.

4. The following findings suggest that vibratory sensitivity is probably more important than tactual sensitivity in the use of the Gault-Teletactor for the interpretation of speech and music.

a. The tongue, which is presumedly quite sensitive to touch but not to vibrations, is not very sensitive to vibratory stimuli from the Gault-Teletactor.

b. The finger nail, which we would expect to find relatively more sensitive to vibratory stimuli than to tactual, is quite sensitive to the Teletactor. The inference is that the vibratory sense and not touch is responsible for the greater sensitivity of the nail to the Teletactor.

c. Sensitivity is not markedly decreased by the wearing of a kid glove. The opposite is obviously true for the sense of touch.

d. The back of the finger which contains many times fewer touch spots than the palmar surface is no less sensitive to the Teletactor. This lack of correlation between sensitivity and the distribution of touch spots suggests the predominant role of the vibratory sense.

e. The sensitivity is not changed by the use of a point instead of the diaphragm stimulating several square centimeters.

f. The sole of the foot is less sensitive than the palm of the hand. Since previous experimeters have found the lower extremities (1) less sensitive than the upper extremities to vibratory stimuli, we would expect this result if vibratory stimuli played a dominant role.

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Northwestern University
Evanston, Illinois

ON THE INTENSITY CRITERION FOR VIBRO-TACTILE DISCRIMINATION

LOUIS D. GOODFELLOW AND MILLA L. ILIEVA

Experiments reported by Robert H. Gault (3, 4, 5) and his research staff (6, 7) show that deaf observers (and normally hearing observers with hearing excluded)[1] are able to make exceedingly fine discriminations of vibratory patterns by touch. Teachers of the deaf (1, 2) who have used the Gault-Teletactor make the same report (10). The suggestion has come from various sources that these discriminations are possible only because of the capacity of the observers to differentiate small intensity variations. We are inclined to think that our observers respond to more than mere intensity differences, and in the following paragraphs we will present the experimental data which has led us to that conclusion.

From data collected for another experiment,[2] we have constructed Table 1. It shows the aggregate scores of two deaf and two normally hearing observers as they tried to identify five vowel and diphthongal sounds by their vibratory patterns. Before the test, each observer was given three one-half hour periods of practice in identification of these five sounds by touch. The test consisted of one hundred presentations of each vibratory pattern, or five hundred patterns in all for each observer. Each time the observer reported which of the five sounds he thought it was.

The responses are summarized in Table 1.

TABLE 1

SUMMARY OF OBSERVERS' RESPONSES TO THE VIBRATORY PATTERNS OF FIVE VOWEL AND DIPHTHONG SOUNDS FELT ON THE TELETACTOR. (Based on 500 trials by each of two deaf and two normally hearing observers.)

Sound produced	Observers' responses				
	A	E	I	O	U
A	280	48	24	23	25
E	48	239	55	36	22
I	24	52	284	19	21
O	44	36	24	264	32
U	28	19	21	28	304

[1]We frequently use normally hearing persons in our experiments, especially when we wish to compare our data on vibratory phenomena with auditory phenomena. For a description of our apparatus and technique for controlling audition, see Gault and Goodfellow (6).

[2]After the construction of the revised form of the Gault-Teletactor in 1933, it seemed important to repeat many of Dr. Gault's old experiments, particularly the work on the identification of vowel and consonant qualities by touch. Such projects are now well under way in this laboratory.

Table 1 shows, for example, that when *A* was produced it was correctly identified 280 times out of the 400. The vibratory pattern of *A* was interpreted as *E* 48 times, as *I* 24 times, etc. These sounds were produced from a victrola record made from the voice of an instructor in the School of Speech. (The electric pick-up of the phonograph was connected directly to the Teletactor amplifier.) The electrical energy activating the Teletactor receiver was measured, and the mean power for each pattern is given in Table 2. An index to the relative difficulty of the

TABLE 2

TABLE SHOWING THE RELATIVE DIFFICULTY OF THE FIVE VOWEL AND DIPHTHONGAL PATTERNS AND THE RELATIVE INTENSITY OF EACH

Vibratory pattern presented	Number of correct identifications (corrected for guessing)	Intensity (energy delivered to Teletactor)
A	62.5%	.27 Milliwatts
E	50.0%	.08 "
I	61.3%	.17 "
O	57.5%	.46 "
U	70.0%	.12 "

five patterns can be obtained from the number of times each was correctly identified as shown in Table 1. These figures have been reduced to a percentage basis and corrected for guessing by the formula

$$S = R - \frac{W}{n-1}$$

where R represents the number of correct identifications, W the number incorrect, and n the number of possibilities.

An analysis of the confusions made by our observers gives an index to the similarity or difference of any two patterns. Table 3 shows the number of times confusions were made for each possible combination of patterns. For example, opposite the heading *A-E* is the number "96" indicating that *A* was interpreted as *E* or *E* as *A* 96 times out of the 800 times *A* and *E* were presented. Obviously, the two patterns most frequently confused must be most similar. The second column shows the intensity ratios of the patterns as indicated by the mean power activating the Teletactor receiver unit.

We infer from the data of Table 3 that *E* and *I* must be the most similar of all patterns since they were confused most frequently (107 times)

TABLE 3

RELATIVE SIMILARITY OF THE VIBRATORY PATTERNS AS INDICATED BY THE NUMBER OF CONFUSIONS MADE AND BY THE INTENSITY RATIOS

Patterns being compared	Number of times confused	Ratio of the intensities of the two patterns
A-E	96	2.4
A-I	48	1.5
A-O	67	1.7
A-U	53	2.2
E-I	107	2.1
E-O	72	5.7
E-U	41	1.5
I-O	43	2.7
I-U	42	1.4
O-U	60	3.9

whereas *E* and *U* must have the most distinctive characteristics since they were confused least of all (41 times). If, now, as our critics suggest, intensity differences were the only criteria by which our observers differentiate vibratory patterns, we should expect to find that the patterns most frequently confused were of similar intensities. For example, the ratio of the intensities for *E* and *I* (the most difficult pair to differentiate) ought to be low and the ratio for *E* and *U* (the pair most readily distinguished) ought to be relatively high. Such is not the case. The vibratory patterns of *E* and *I* (the most difficult pair) have a greater difference in intensity than the patterns of *E* and *U* (the simplest pair to differentiate). The same thing is true for other pairs. For example, the vibratory patterns of *I* and *U* are apparently quite different since they were easily differentiated by our observers, although their intensities as objectively measured are more nearly alike than the intensities of the vibratory patterns of *A* and *E* which (judging from the number of times they were confused) are quite similar. We must point out, however, that there are cases in which a positive correlation does exist between similarity of patterns as measured by intensity and similarity as estimated by the difficulty our observers experienced in differentiating them. In these cases intensity may be the cue used by our observers in identifying the patterns. Nevertheless, this does not invalidate the original statement that intensity is not the only criterion by which persons differentiate two vibratory patterns. Although intensity is undoubtedly one of the factors in all vibratory discrimination, we have pointed to cases in which intensity differences were apparently subordinated to other cues.

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Northwestern University
Evanston, Illinois

APPARATUS

QUANTITATIVE RECORDS OF CUTANEOUS SECRETORY REACTIONS[1]

CHESTER W. DARROW

That a satisfactory method for continuously recording the amount of perspiration from limited skin areas has not already been developed is remarkable, considering the psychological importance of perspiration as an indicator of emotional changes and the medical importance of perspiration as a clinical symptom. The total weight loss from the body due to the elimination of the products of respiration and the evaporation of moisture from the skin has been studied by Benedict, Miles, Freeman, and others, using a delicate balance showing slight changes in the subject's weight. The *per cent* relative humidity of air next the skin has been determined in various ways, for example, by Wetherell (1905), using spirals made of strips having hygroscopic material on one side. He also employed the color change in filter paper soaked in cobalt chloride as an indicator of relative humidity. The amount of perspiration from finite skin areas was determined by Waller (1894), using cups filled with calcium chloride to absorb moisture over 10-minute periods. Kuno and Ikeuchi (1927) measured the amount of perspiration from various skin areas by passing dry air through celluloid cups attached to the skin and collecting the moisture taken up in 5-minute periods, by passing the air through calcium chloride. The writer (1927) has succeeded in obtaining continuous records of simultaneous changes in sweat secretion and in the galvanic skin reflex. The variations in perspiration were indicated by the effects of the evaporated moisture upon the resistance of fibers coated with an ionizable deliquescent substance. Williams (1931) has suggested a somewhat similar method.

Although the ionized fibers, as originally used, gave us clear records of slight changes in the amount of perspiration, the method was not sufficiently stable to permit reliable calibrations. In order to state definitely what a given skin resistance or a given skin-resistance change means in terms of secretion and to answer many fundamental questions regarding the interpretations of the galvanic skin reflex, it is necessary to have a device which will not only be immediately responsive to slight changes in moisture on the skin, but which may also be calibrated in milligrams per second for a unit area. Although a gradual shift in sensitivity is characteristic of the following modification of the ionized fiber device and frequent calibrations are necessary, the

[1]Studies from the Behavior Research Fund and Institute for Juvenile Research, Series B, No. 195.

method, nevertheless, makes possible the quantitative measurements which are desired.

A loose bundle of spun glass fibers is enclosed in a glass tube terminating in metal ferrules as, at *A*, in Figure 1. This tube of fibers is, in shape, size, and function, similar to the small "grid leak" familiar in radio construction, save that there is a hole in each end to permit the passage of air. The fibers are repeatedly coated with a hygroscopic conductive solution (1 cc. of glycerine and 5 grams of sodium sulphate in 50 cc. of water) until they have a resistance of from 80 to 100 megohms when exposed to a current of dry air. The resistance of the fibers is recorded by a Wheatstone Bridge B, of the same form as the *Behavior Research Resistance Box* (Darrow, 1932), save that the values of the resistances are 1000 times as high and that a potential of 180 volts is employed. This circuit provides continuous quantitative measurements of the resistance of the fibers when they are exposed to varying amounts of moisture in the air drawn from the surface of the skin.

The arrangement of the apparatus is indicated in Figure 1. Dry air, drawn through a calcium chloride bottle *C*, for drying, passes over the surface of the skin inside a rubber cup, *D*, and then is drawn through the

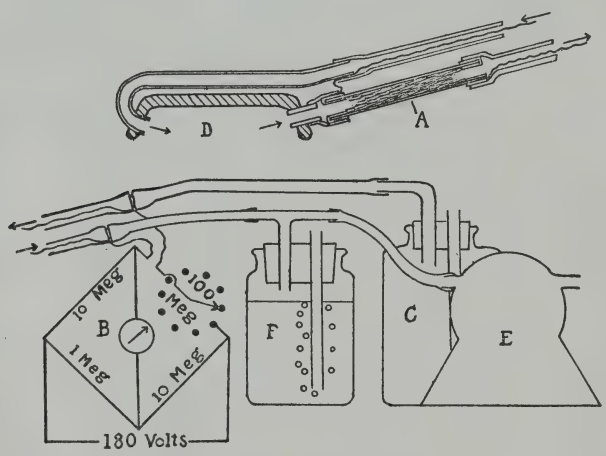

FIGURE 1

PERSPIRATION RECORDING APPARATUS

A, tube of hygroscopic fibers (¾ size); D, rubber cup of size and shape of electrodes used for galvanic skin reflex; B, Behavior Research Resistance Box with adjustments in the X-arm of the bridge; C, calcium chloride drying bottle; E, motor driven pump; F, air valve maintaining constant pressure in system.

tube of conductive fibers. If the air is moist, it gives up moisture to the fibers, increasing their ionization and electrical conductance. If the air from the skin is dry, it takes up any free moisture from the fibers, thus decreasing their conductance. A constant current of air is maintained through the system by means of a pump, *E,* and an air valve, *F,* as shown in the diagram. Figure 2 shows a sample record of perspiration taken along with records of certain other physiological changes. When the fibers are calibrated against a device delivering measured quantities of moisture into the stream of air going to the fibers, we obtain curves such as are shown

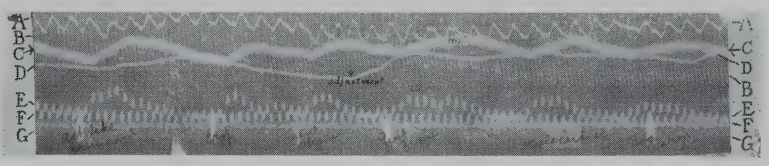

FIGURE 2

SAMPLE RECORD OF PERSPIRATION AND OTHER PHYSIOLOGICAL CHANGES

A. Respiration.
B. Blood pressure—left ankle.
C. *Perspiration by hygroscopic fiber method—palm of left hand.*
D. Perspiration by "wet-dry bulb" method—palm of left hand.
E. Galvanic skin reflex—palm of right hand.
F. Galvanic skin reflex—back of right hand.
Vertical white lines are second marks.

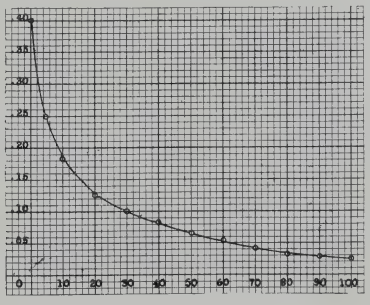

FIGURE 3

MEGOHMS RESISTANCE OF HYGROSCOPIC FIBERS

Milligrams moisture per second required to maintain the resistance of a set of hygroscopic fibers at various levels. Each plotted point is based on three readings of the pipette (described in a succeeding article) made with the assistance of a lens and a stopwatch. The ordinate line is plotted in milligrams of moisture per second, while the abscissa is plotted in megohms resistance of hygroscopic fibers.

in Figure 3. With the tube of fibers arranged adjacent to the cup on the hand and with the slow rate of air flow employed by us (a negative pressure of 1 cm. Hg.), the device has a latent time of about 2 seconds. Reactions closely follow the accompanying galvanic changes and correlate with them in magnitude (when the reactions all occur at the same general resistance level). Having read the resistance of the fibers at a given point in a subject's record, it is only necessary to refer to the calibrated curve obtained for those fibers to determine the milligrams of perspiration per second given off from the area under examination.

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Behavior Research Fund and Institute for Juvenile Research
Chicago, Illinois

A "WET AND DRY" THERMOPILE FOR RECORDING PERSPIRATION[1]

Chester W. Darrow

An application of the principle of the "wet and dry" bulb thermometer gives promise of giving a satisfactory measure of the less rapid perspiration changes.

Briefly, the device consists of a pump for drawing air at a constant negative pressure from the skin surface first over the junctions on one side of a compound thermocouple, and then back along the junctions on the other side. On the latter series of junctions rests a narow strip of linen, kept moist by capillary action from water in a small reservoir. The only difference in temperature between the two sets of junctions is that due to the cooling effect of the evaporation of moisture from the linen strip. The amount of such evaporation is determined by the dryness of the stream of air passing over it. The potential delivered by the thermopile as indicated by a delicate galvanometer and a potentiometer thus becomes a sensitive indicator of changes in the humidity of the air drawn from the surface of the skin.

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[1]Studies from the Behavior Research Fund and the Institute for Juvenile Research, Chicago. Series B, No. 196.

In more detail, the device is constructed as follows: A thin bakelite strip *A* (Figure 1), is wound with fine constantin wire. One side of the flat coil

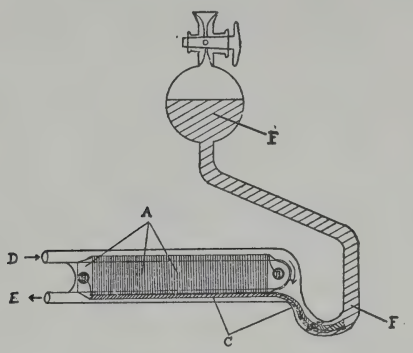

FIGURE 1
"WET-DRY" THERMOPILE
A, Bakelite strip wound with constantin wire; B, binding posts; C, linen strip; D, air intake; E, air exit; F, water in reservoir.

thus constructed is plated with copper, after the general method described by Downing, Gerard, and Hill (1926) for the wedges in their thermopile for recording heat production of nerves. This procedure gives a line of copper-constantin junctions along one edge of the strip, and of constantin-copper junctions along the other edge. The thermopile is then covered with a thin layer of celluloid varnish. Along the lower edge a narrow strip of linen, *C,* is fastened, with one end extruding to dip into a reservoir of water, *F,* maintained at a uniform level by atmospheric pressure. The whole device is then inclosed in sheet celluloid with the center clamped together so as to produce two tubes, one along the upper edge, the other along the lower edge, the two being connected as shown, to permit the ready passage of air from the upper to the lower tube. Air entering the upper tube at *D* passes along the dry edge of the thermopile, then downward and along the wet edge. We obtained a current of 50 microamperes in our apparatus when using dry air. A potentiometer has been used to supply a counter E. M. F. to balance that from the thermopile when employing a highly sensitive galvanometer. The device has been calibrated according to methods described elsewhere, and has been shown to approximate a straight line relationship between the current generated and the amount of moisture in the air. The device is much more sluggish in its action than are the ionized fibers which we have also employed for recording the moisture on the skin.

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Behavior Research Fund and Institute for Juvenile Research
Chicago, Illinois

CALIBRATION FOR A RECORDER OF PERSPIRATION[1]

CHESTER W. DARROW

Continuous measurement of the perspiration on small areas of skin requires not only a highly sensitive device for indicating changes in moisture, but also a satisfactory method for quantitatively calibrating the apparatus. Three of the methods used by us for calibration deserve mention: (1) A microsyringe delivering a constant flow of water by the pressure from a slow moving screw drive; (2) an electrolytic micropipette in which liquid in a pipette is displaced at a known rate by gas electrolytically dissociated by a minute controlled electric current; and (3) a small tightly wound electrically conductive coil at the tip of a finely graduated pipette. The coil is heated by a controlled electric current to supply the heat of exaporation for the water within and on its surface. The first two devices themselves require calibration before they may be used to calibrate the perspiration recorder. The third method permits reading with a stop watch the time required for the evaporation of successive ten milligram quantities of water supplied to the moisture recorder, and permits ready determination of the meaning of deflections of this latter apparatus in terms of milligrams of moisture per second.

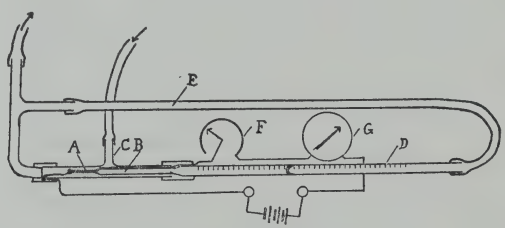

FIGURE 1

MOISTURE CALIBRATOR

A, tightly wound water-containing coil; B, pipette; C, T-tube; D, graduated pipette; E, tube equalizing pressure above and below water in pipette; F, rheostat; G, ammeter.

[1]Studies from the Behavior Research Fund and the Institute for Juvenile Research, Chicago. Series B, No. 197.

The device is schematically represented in Figure 1. A small tightly wound coil *A*, 1 mm. in diameter, closed by a screw at one end is fastened at the other end to the tip of a small glass pipette, *B*. The coil and pipette are sealed inside a T-tube, *C*, and connected by a tube to a graduated micropipette, *D*, containing distilled water. Air from a drying bottle is sucked into the branch of the T-tube, past the coil of wire, and thence out to the moisture-recording apparatus. A connection, *E*, to the upper end of the pipette equalizes the air pressure above and below the contained water. The angle of incline at which the pipette is set will determine the pressure of the liquid within the water containing coil. When the wire leads to the coil are connected to a storage battery through a rheostat, *F*, and an ammeter, *G*, the rate of evaporation of water at the tip of the pipette may be controlled. A stopcock-controlled-by-pass (not shown) around the coil may be added for readings on extremely dry air. The rate at which evaporation occurs with a given setting of the device may be determined by reading successive levels of the liquid in the graduated pipette. By means of a prepared table, time for 1/100 of a cc. (10 milligrams) is readily translated into milligrams of moisture per second.

Behavior Research Fund and Institute for Juvenile Research
Chicago, Illinois

THE SIGNIFICANCE OF SKIN RESISTANCE IN THE LIGHT OF ITS RELATION TO THE AMOUNT OF PERSPIRATION[1]

(Preliminary Note)

CHESTER W. DARROW

Plotting skin resistance against the amount of perspiration measured on a corresponding skin area shows that perspiration tends to vary as the reciprocal of the resistance, or as *conductance*. In other words, at high resistance levels large-ohms reactions to stimulation are accompanied by very slight changes in physiological activity, and at low resistance levels, small changes in resistance are accompanied by large changes in physiological activity. Assuming that the amount of perspiration is an index not only of the degree of physiological activity of the sweat glands, but also to some extent of the intensity of the concomitant general physiological and psychological activity, it follows that the use of skin resistance, measured in ohms, may be expected to have only a very rough relationship to various physiological or psychological variables. Numerous investigators[2] have at-

[1]Studies from the Behavior Research Fund and the Institute for Juvenile Research, Chicago. Series B, No. 198.

[2]See Darrow and Heath (1, pp. 286-287).

tempted a rough compensation for the apparent curvilinear relation of skin resistance to various forms of activity by translating resistance changes in ohms into percentages of the resistance level. Evidence that the activity of the sweat glands corresponds with a reciprocal rather than with a percentage resistance curve suggests that the use of percentages does not offer sufficient correction.

On the other hand, the expression of galvanic activity in units of conductance not only offers correction for the reciprocal relation of sweat gland activity to resistance level, but also promises considerable increase in the facility of manipulating galvanic measures. The gemmho[3] or micromho, the reciprocal of the megohmic resistance, provides a convenient working unit. According to this measure a subject at 10,000 ohms will have 100.0 micromhos conductance; at 20,000 ohms, conductance will be 50.0 micromhos; at 50,000 ohms, it will be 20.0 micromhos; at 100,000 ohms it will be 10.0 micromhos; and at 200,000 ohms, it will be 5.0 micromhos. With this unit, reactions to stimulation are expressed simply as the difference between two measurements of conductance. The use of percentages is no longer necessary. A table of reciprocals or the use of a reverse scale in a slide rule simplifies the translation of resistance into units of conductance.

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Behavior Research Fund and Institute for Juvenile Research
Chicago, Illinois

AN EFFICIENT PHOTO-RECORDING GALVANOMETER[1]

Chester W. Darrow

A relatively low-priced, highly sensitive, quick-acting galvanometer for photographic recording of the galvanic skin reflex, sweat, and other relatively slow physiological changes may be readily constructed from a small

[3]More exactly, it varies as the *variable* conductance. It appears necessary to subtract from the total apparent resistance the resistance of the electrodes and also the basic or non-variable resistance of the circuit through the body, thus:

$$\text{Variable conductance} = \frac{1}{\text{Total res.—non-variable res.}}$$

[1]Studies from the Behavior Research Fund and the Institute for Juvenile Research, Chicago. Series B, No. 199.

two-inch microammeter. The modified instrument may be set up independently with a suitable lens and lighting system, and will give continuous records of the galvanic skin reflex practically indistinguishable from those of a string galvanometer. It may furthermore be arranged in such a way that the pointer is in the focus of the ocular of standard string-galvanometer equipment, thus providing an accessory galvanic record useful in many forms of investigation.

A two-inch microammeter (range, twenty microamperes) having a magnet of the new highly magnetic cobalt steel and costing in the neighborhood of $25 is employed. The instrument is removed from the case and the binding posts are moved from the back to a suitable position on the side of the instrument case. A hole is turned in the back of the case large enough to fit the ocular tube of a string galvanometer or to permit a small condensing lens to be focused through the center of the horseshoe magnet. Before replacing the movement in the case the pointer and counterbalances are carefully removed from the moving coil. In the place of the regular pointer, a very fine spun glass fiber (tubing) is cemented, the size of the fiber depending on the magnification of the eyepiece to be employed. A microscope eyepiece giving a magnification of 15 to 20 is recommended. This eyepiece is either cemented to the outside of the glass of the instrument case, the glass fiber being adjusted to fall in the focus of the lens, or, preferably, it is set within a hole cut through the glass permitting adjustment of the focus.

This arrangement gives a coil galvanometer with moving parts of minimum mass and with a very small amount of movement covering the active range. This makes possible a combination of rapidity of action and sensitivity unusual in this type of instrument. When combined with a suitable shunt for damping, the deflections are practically dead beat. The natural period of the undamped instrument is two double vibrations per second.

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BOOKS

J. B. RHINE. *Extra-Sensory Perception.* (Foreword by W. McDougall and Introduction by W. F. Prince.) Boston: Boston Soc. Psychic Res., 1934. Pp. xiv+169. $2.50.

Dr. Rhine's book is one of the most important contributions to psychical research yet published; it seems to the reviewer to be the first really thorough, substantial, and competent investigation of telepathy and clairvoyance by the experimental method. The *finesse* of the experimental method is perhaps not equal to that of the method used by the Gröningen experimenters (*Compte rendu,* First and Second Congresses of Psychical Research), nor is the psychological analysis of the conditions attending success as thorough and illuminating as that of Warcollier (articles since 1921 in the *Revue Métapsychique*); but at our present stage in the history of the subject these are not our chief needs. Our first need is to get the phenomena *under control,* with a group of subjects willing to work for a long time with a standard method upon which planned systematic variations can be imposed, the results being uniformly compared against chance expectation. Dr. Rhine has achieved all these things. Since 1930 he has been building the group and the method; and the phenomena appear to be under about the sort of control which a complicated process, partly subject to voluntary guidance, might be expected to show. Any one wishing a sane avenue of approach to psychical research should read this book. If, in fact, the reader forms any opinion of the book merely on the basis of the review, he and the reviewer have both done a bad job; for the subject bristles with difficulties, and a review is useful only in directing the serious reader to this first-magnitude effort to meet, control, and banish these difficulties.

The subjects for these experiments were recruited among students at Duke University, and those with whom the bulk of the report deals have labored at the task for a matter of months or years. The brief personality-sketches and photographs of the subjects add definitely to the vividness and realism of the report. The material used consists uniformly of cards upon each of which has been stamped one or another of five symbols: O □ + ★ =. Packs of twenty-five cards (each pack containing five cards bearing *each* of these symbols, but all shuffled together) are the standard units of experimental material. The chance-expectation is five in twenty-five right, and with this the results are compared. Usually the results with a given subject, under a given set of experimental conditions, are grouped into large blocks, and the results are treated in terms of the deviation above chance-expectation divided by the probable error of this

deviation. (These anti-chance probabilities are mostly of the order of billions, trillions, etc., against one.)

The traditional routine method of dealing with such material would be to have the "agent" look at the card, and the "percipient" record his guess. Dr. Rhine, however, has run an enormous number of series in which, after being shuffled and laid down, the cards are seen by no one, and another great number in which the agent visualizes the card-figures without depending on any physical stimulus-object. These are called experiments in "pure clairvoyance" and "pure telepathy," respectively. The results may be summarized in the most general way by saying that: (1) most subjects do not make scores much above chance-expectation at the beginning; (2) after a few series a successful group emerges from among the subjects, a given subject maintaining for months (or even years) a characteristic score of say eight or ten in twenty-five, with many series running twenty or better in twenty-five—each subject's excess above chance score is gauged individually in terms of the billions, etc., against one of a true value greater than zero; (3) subjects who are good at telepathy are, in general, good at clairvoyance, under the conditions of the experiment, and the two functions seem to be closely linked in many ways; (4) when the phenomena are under fair control, success increases with distance and with walls, doors, etc., interposed. Some of the best series were made at long distance (the author reproduces a photograph of the buildings in which some of the experiments were conducted; one of the best performances was done in a room facing away from, and a hundred yards from the location of the cards which were to be guessed). This sort of result appears, as in the work of several other experimenters, to be due primarily to the freedom which long distance gives from the distraction which a nearby agent, or stimulus-object, sets up. It is to be regretted that the results of the long-distance series, after being statistically treated from the point of view of anti-chance, are sometimes pooled with those of more casual experiments which are not in themselves proof against interpretation in terms of hyperaesthesia, so as to give still higher anti-chance figures. Since, however, the long-distance and other carefully guarded experiments attain, in their own right, anti-chance values of billions and trillions, the objection carries more weight on aesthetic than on scientific grounds.

Among the psychological conditions favoring success appears, with special prominence, the condition of "abstraction," the shutting out of disturbing stimuli; perhaps a better word is "concentration," but without the directed thinking which the latter term implies. Since this state of detachment from the distractions of the immediate surroundings requires effort, it is not surprising to find that caffeine notably improves the scores, and that sodium amytal greatly depresses them. Here and there are evidences of "fatigue" effects, but there are some very long series without falling-off,

and Dr. Rhine doubts the suitability of the term fatigue. Confidence, good humor, and personal rapport are favorable, as is to be expected if we are really dealing with delicate and difficult mental work.

There is some evidence, however, that success in the telepathic series does not depend exclusively upon the percipient; there is such a thing as a good "sender," in relation to any given percipient. Perhaps, too, there are good all-around senders, though the point is not established.

There are two minor criticisms which may perhaps seem major before one has read the book; after reading it, one will see that their importance is slight: (1) The method of shuffling the cards is not described. A really thorough shuffling might take a long time, and a percipient might follow here and there (quite unconsciously) an order which had been followed before. If one had made a high score in a previous series (being told that it was high when the series was concluded), this would introduce some error. This general line of criticism does not count much against extra-sensory perception, but a good deal that Dr. Rhine classes as clairvoyance may well have been telepathy from an agent who had some recall of a previous series, or who, despite shuffling the cards, really had more than a chance acquaintance with the way the cards lay. In any event an automatic shuffler should have been used. (2) In the telepathy series the agent was free to choose the symbols as he liked. The agent chose not one, but a series of five; i.e., he made a new set of choices five times in the course of a series of twenty-five. He undertook to avoid all system, all clues or plans which might be guessed by the percipient. It is not at all clear, however, that in this way he could consistently avoid being "out-guessed"; or, more likely, avoid falling into habits which his partner was unconsciously following. When the two partners are intimately acquainted, this source of error becomes rather troublesome. It may be avoided by having the choices made by a roulette wheel, the numbers of which stand for symbols, the wheel being returned to the zero point after it has made its choices; or (if clairvoyance for the wheel is to be avoided at all times), a code sentence may be drawn from a book by any chance device, and the letters of the sentence be translated into symbols by any prearranged scheme.

The chief omissions in the work are, from the reviewer's standpoint, a bit more important: (1) The subjects' own descriptions of their mental condition during the experiments are not so presented as to permit full comparison of what was happening during the runs of high success with what was happening when success was momentarily down to the chance level. (2) Since there is a good deal of evidence that color and form perception are two very different functions, it is to be regretted that all of the subjects were tried only on form-perception. Auditory material and some other types of material could easily have been used also; everything could have been grouped in sets of five so as to make the statistics comparable.

Any one, however, who has labored to get any of these puzzling phenomena under control will realize how amazing Dr. Rhine's achievement is. Most of the published studies in experimental telepathy are crude and amateurish, and—more serious still—they are too brief, or statistically too confused, to permit a clear view of what is happening. The work of Estabrooks and of Brugmans was too brief, and that of Warcollier not susceptible to statistical evaluation. Troland's, Coover's, Soal's, and my own studies, though sometimes attempting statistics, never got the phenomenon under experimental control, and therefore throw about as much light upon it as a study of the fauna of Greenland would throw upon the causes of yellow fever.

In comparing Dr. Rhine's book with all previous experimental studies, I should venture the statement that what counts in science is what is witnessed in such amount and under such conditions that its laws become decipherable, and that the present volume is the first sustained attempt to gather the material under defined conditions with true comprehension of the nature of the difficulties and the ways of surmounting them.

Our modern material civilization is depending more and more upon a power which a few centuries ago amused those who studied the action of amber upon silk. The phenomena were bizarre, but obviously of no theoretical or practical importance. The analogy is close. The twenty-fifth century will give Rhine's experiments the importance they deserve in the history of science.

GARDNER MURPHY.

Columbia University
New York City

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